1.0

1.1

1.25   1.4   1.6

2.8   2.5
3.2   2.2
3.6
4.0   2.0

1.8

MICROCOPY RESOLUTION TEST CHART
NATIONAL BUREAU OF STANDARDS 1963-A

FOR FURTHER TRAN

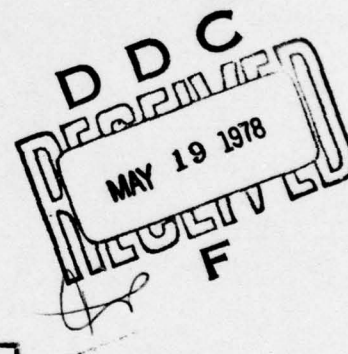# INVESTIGATION OF THE TACTICAL CONTROL

OF COVER AND EXPOSURE AND ITS

RELATION TO PREDICTED COMBAT RESULTS

28 FEBRUARY 1977

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Submitted to:

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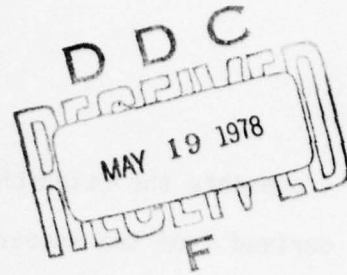## Ann Arbor, Michigan

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PREFACE

This is the final report of research by Vector Research, Incorporated, under contract number DAAG39-76-C-0027. The research was concerned with two topics in the broad area of the interactions of terrain and tactics in small unit armored and mechanized combat engagements. One part, described in part I of this volume, addressed the problem of identifying specific quantitative elements of tactical exposure which predict combat results (as estimated by combat models). The second part, described in part II of this volume, examined the degree to which systematic interpersonal variations in tactical decision making may cause variations in engagement conditions, and therefore possibly in combat results.

The results of the first part strongly suggest that fine details of the control and coordination of the exposure periods of the individual weapon systems engaged in a battle are a principal determinant of the predicted combat results of the engagement. The results indicate the need for further research in the area of detailed tactical control of fire and maneuver, including field experimentation concerned with the feasibility of and benefits to be derived from increased degrees of maneuver coordination in small unit armored or mechanized combat. The research conducted and the results obtained are described in the five chapters of part I. Chapter 1 presents the background of the current research. Chapter 2 describes the specific research problem addressed in this study. Chapter 3 discusses the design of the research. Chapter

4 presents the research results, and chapter 5 summarizes the conclusions derived from the research. Readers interested in a brief presentation of the research should read chapters 2, 4, and 5.

The results of the second part of the research, which involved experiments conducted with Army officers at the Armor and Infantry Schools, show a large variability in the design of tactical organizations for combat, but no significant systematic officer-to-officer variation in this or any other of the major tactical variables examined. The results suggest some minor changes in Army study methodology to improve validity, The organization of part II is exactly parallel to that of part I, having five chapters with similar contents. In this part, however, readers interested only in a brief summary of the research are encouraged to read chapter 1 as well as chapters 2, 4, and 5, omitting only chapter 3.

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ACKNOWLEDGEMENTS

In addition to Robert L. Farrell, the principal investigator, several
other VRI staff performed significant research efforts on this project.
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The assistance and support provided by the study sponsor's representa-
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of the Army was invaluable. Without the support of the Army Armor and
Infantry schools and the approximately 60 officers there who partici-
pated in the experimental work, the second part of this study could
not have been performed.

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FIGURES

PART I

DETERMINING MEASURES OF TACTICAL EXPOSURE WHICH

CORRELATE WITH COMBAT RESULTS AS PREDICTED

BY COMBAT MODELS

PART I, CHAPTER 1: BACKGROUND

For the last decade or slightly longer, military operations analysts
in many countries have been measuring statistics concerning terrain
line-of-sight opportunities. Measurements have been conducted by the
French, Germans, British, and Americans, with some of the most sophisti-
cated measurements being made in Phase I of the recent US TETAM field
experiments. These experimental measurements have been designed to
provide basic data for analyses of alternative weapon system designs,
force mixes and structures, and force sizes. The statistical measure-
ments taken have generally centered on the distribution of the number and
length of exposures of attackers to defenders (or vice-versa) on tactically
selected advance routes approaching a basically linear defensive position
occupied by several defender weapon systems.

A summary of some of the major history of such measurements can
be found in [Hardison, 1974]. As is explained in that paper and the
other papers of the seminar at which it was given, the principal concerns
behind the measurements of terrain characteristics are two:

(1) the direct use of the resulting measurements in
     studies or analyses of the value of various weapon
     system designs (generally using simple statistical
     models or "time line" simulations as the basic
     analytic tools);

(2) the analysis of the diversity of terrain classes

which may be expected, so that reasonably representative terrains may be chosen from each class for more detailed analysis and use in high-resolution analyses of the potential contributions of weapon system or force changes to force effectiveness. (These detailed studies typically use a digitized representation of a specific piece of terrain chosen as a representative of a larger class of possible engagement sites.)

A typical aggregate set of exposure statistics from several experiments is shown in figures 1 and 2. More detailed data, including a breakdown of the distribution of exposures by range, are available from several of the experiments. Of course, given the use for which the data are designed, these measurements have been made on the basis of a major assumption:

> *Terrains which are similar with respect to measured line-of-sight statistics produce similar distributions of combat results[1] when fought on by identical forces using constant tactics.*

If this assumption is not true for a particular set of line-of-sight statistics, then it is clear that a different set of statistical measurements of exposure is needed--one which will make the assumption true. That is, any classification of terrain by exposure characteristics is useful only to the extent that it provides information regarding the potential outcomes of combat on all terrains in a class; to the extent

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[1] As predicted by high-resolution combat models.

Study	Number of Initiations of Line of Sight	Mean Segment Length (meters)
NATO Range Study	3.5^1	385
HELAST I	4.2	393
HELAST II	7.0	59
Lost Horizon	2.7	326
Experiment 11.8		
Fulda Gap	4.0	357
North German Plain	3.0	183

FIGURE 1: EXAMPLE EXPOSURE DATA

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[1]Data not available from this experiment, estimated on basis of 11.8 Fulda data.
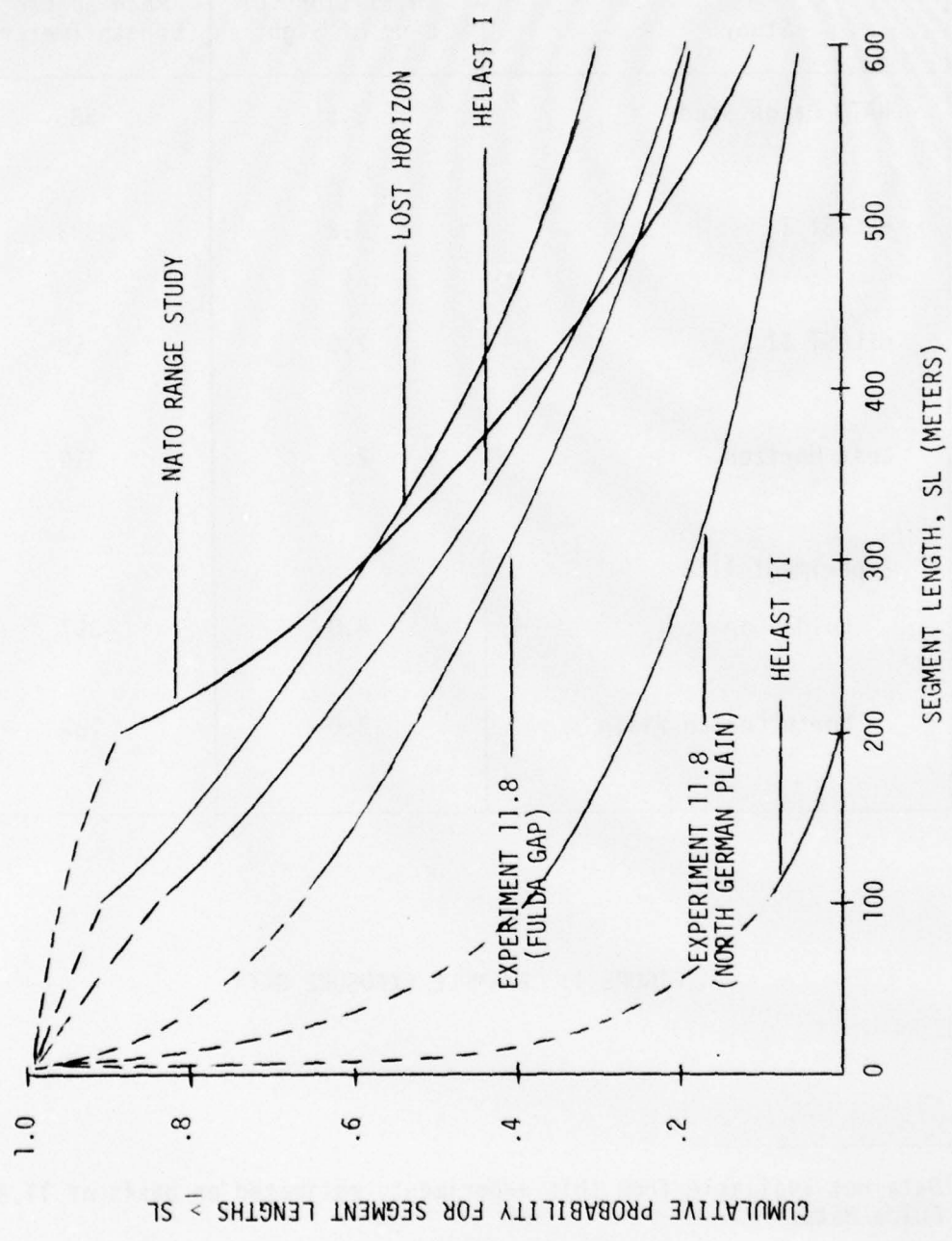
FIGURE 2: EXAMPLE TERRAIN SEGMENT LENGTH DISTRIBUTIONS

that it leaves the outcomes uncertain, a change or refinement in the classification is necessary.

Starting in 1970, a series of experiments have called this assumption into serious question. The research program reported on here was designed to go beyond these negative results by designing and investigating alternative line-of-sight classification statistics which would be more closely associated with combat results, structuring the determination of the new line-of-sight statistics in terms of the interaction of terrain (land form and vegetation) and tactics (weapon siting and maneuver, etc.) and evaluating the contributions of interpersonal variations in tactical interpretations to line-of-sight and combat result variability. The remainder of part I of this report discusses the research on the new measures of exposure, and part II discusses the evaluation of interpersonal variability. Within part I, the remainder of this background chapter reviews the previous results, chapter 2 provides a concise description of the goals of this research, chapter 3 describes the activities undertaken in this research, chapter 4 discusses the results of this phase of the project, and chapter 5 summarizes the main conclusions.

The initial 1970 work [VRI, 1970] analyzed whether or not combat results for line-of-sight realizations drawn at random from a single statistical population would be similar. A typical case from the analysis is shown in figures 3 and 4, where five different line-of-sight realizations drawn at random from a single statistical population were played in a high-resolution deterministic combat model. As can be seen from the figures,

Group	Weapon Type	Initial Number	Realization Number				
			1	2	3	4	5
Red							
1	APAT	1	0	0	0	0	0
2	APC	3	2.52	0	.40	1.65	1.13
3	MBT	3	1.74	1.11	.27	0	.41
4	ATRL	3	0	0	0	0	0
5	ATGM	1	0	0	0	0	0
6	ATRL	3	0	0	0	0	1.17
7	MBT	3	0	0.76	0	0.41	0.52
8	APC	3	0	0	1.83	0.63	0
9	APAT	1	0	0	0	0	0
	(% Red MBT Survivors) =		.29	.31	.045	.068	.155
Blue							
1	MBT	5	2.05	0.98	2.94	2.07	1.70
2	APC	8	7.46	6.71	6.43	7.02	6.47
3	MRT81	3	3.00	3.00	3.00	3.00	3.00
4	APC	8	5.89	5.63	6.55	4.93	6.41
5	MBT	5	2.57	0	2.01	1.88	0.77
	(% Blue MBT Survivors) =		.563	.098	.595	.393	.247

FIGURE 3: SURVIVING FORCES AT THE FEBA FOR
FIVE LOS REALIZATIONS

Analysis Point	Realization Number	Surviving Red MBT	Fraction Surviving Red MBT	Surviving Blue MBT	Fraction Surviving Blue MBT
50% Blue MBT	1	2.33	.39	5	.50
	2	3.33	.55	5	.50
	3	0.33	.055	5	.50
	4	1.98	.33	5	.50
	5	2.99	.50	5	.50
33% Red MBT	1	2	.33	4.86	.486
	2	2	.33	1.44	.144
	3	2	.33	6.04	.604
	4	2	.33	5.02	.502
	5	2	.33	3.07	.307

Initial Number Red MBT = 6
Initial Number Blue MBT = 10

FIGURE 4: SURVIVING FORCES AT FIXED LEVELS OF
ENEMY FORCE SURVIVAL FOR FIVE LOS
REALIZATIONS

there was extremely high variability in survivor ratios, loss ratios, and absolute numbers of systems across the five statistically identical line-of-sight realizations. The case-to-case standard deviation of the number of Red tank survivors is of the order of magnitude of the mean number of survivors.

This particular analysis -- based on random draws from a statistical population -- resulted in similar but not identical line-of-sight statistics in the five scenarios, and it was followed by an analysis of cases with terrain line-of-sight realizations with identical *sample* statistics (instead of random sample statistics drawn from a population with a constant *population* statistic). The results continued to show strong variability comparable to that in the original cases. This remained true even when exposure window length, masked length, and the average number of attackers simultaneously intervisible to a defender were held constant.

These results called into serious question the appropriateness of the particular terrain line-of-sight statistics being used to summarize and classify terrain. However; they were themselves limited in two significant ways:

(1) they were produced using a deterministic combat model, and thus did not compare the intra-terrain variability with the stochastic variation expected simply due to the randomness of combat, and

(2) they confounded and possibly misrepresented the effects of terrain proper (land form and vegetation, etc.) with those of the tactics of deployment and maneuver on the terrain by operating directly on the resulting line-of-sight histories.

In order to be certain that these study limitations did not drive the results, a more detailed study was performed in 1973 and 1974 [Farrell and Freedman, 1975].

The general methodology of that study, sponsored by the Army Materiel Systems Analysis Agency (AMSAA), was as follows:

(1) AMSAA selection of terrain areas for examination. Five areas were selected in each of two general roughness categories. The specific samples were chosen to be as similar as possible from the point of view of the military and operations research analysts selecting them, and to be, as closely as possible, actual alternative locations for the simulated actions of a potential weapon system or force design study.

(2) The design, by AMSAA personnel, of specific defensive positions for two groups of three weapons each and of attack tactics for three platoons of five tanks each. The defensive weapons were assumed to be three tanks and three (mounted) anti-tank missile launchers. The AMSAA staff who participated in this design are staff normally engaged in similar activities for actual studies investigating the operational effectiveness of alternative systems and force designs for both the Army "developer" and "user" communities.

(3) Use of these AMSAA scenarios with representative actual weapon system data in a Monte-Carlo combat simulation to produce combat results. At least 50 replications were run on every scenario in order to achieve tight statistical estimates.

(4) Analysis of the simulation results in order to determine the extent of inter-scenario variance. (Inter-scenario variance included the combined effects of the terrain area selection, the defender location selection, and the attacker route selection.)

The overall approach was to use methods as closely identical as possible to those of actual weapon system and force design studies (which are, in turn, intended to provide as close an image of real combat processes as possible). These methods, used to generate combat results on terrain scenarios as identical as possible, allow relatively strong conclusions to be drawn concerning the impact of terrain. Care was taken at every stage of the process that the terrains and tactics of the scenarios were as militarily real as possible, including the careful design of separate intelligent tactics[1] for each terrain sample investigated. This care was intended to prevent any possibility of confusing realistic sensitivity to terrain with mere sensitivity to mathematical terrain parameters whose variability could be controlled in reality by the military decision-making process.

The results of this research bore out and, if anything, strengthened the earlier conclusions. Specifically, the analysis indicated that:

(1) There is extreme variability (sensitivity) in combat model results as the scenarios (terrain and movement assumptions) are varied, even when this variation is within a class of scenarios chosen for their *a priori* equivalence.

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[1]The term "intelligent tactics" is used in this report to connote tactics that take account of terrain, intelligence about enemy strength and location(s), fields of fire, weapons characteristics, etc.

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(2) This variability can be slightly reduced, but remains extreme (with probabilities of win estimable only within plus or minus 25%) *even if battle results are used to redesign the scenarios.*

(3) The observed variability is generally independent of the degree of detail or sophistication of the combat process models (although the exact combat results on each scenario are not). This is true even when smoke is used to deliberately control line of sight.

(4) This sensitivity does not produce uniform or monotone variations in results for changes in weapon system design parameters, but is such that weapon system or force design choices made on a cost-effectiveness basis may be reversed between equivalent or equally likely terrain and scenario choices.

Figure 5 shows an example of the results of this analysis for five terrains, labelled A1 through A5. Each battle was considered terminated whenever the surviving force ratio was greater than 5 or less than 1 or when the attacking forces reached the defenders.[1] The figure shows for the five terrains the probabilities of the various outcomes and the conditional mean numbers and standard deviations of the survivors for each outcome (overall means and standard deviations of survivors independent of outcome can be computed from this data, but are not presented explicitly). The high variability of the win probabilities is the most remarkable feature of the results and was the strongest determinant of the study conclusions listed above.

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[1]The initial force ratio was 15 to 6 (2.5 to 1).

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SCENARIO	TERMINAL CONDITION	PROBABILITY OF THIS TERMINATION	CONDITIONAL MEAN ATTACKING SURVIVORS	CONDITIONAL MEAN DEFENDING SURVIVORS	CONDITIONAL STAND. DEV. OF ATTACKING SURVIVORS	CONDITIONAL STAND. DEV. OF DEFENDING SURVIVORS
A1	DEF. WIN (RATIO ≤ 1)	.01	4.00	4.00	0	0
	ATT. WIN (RATIO ≥ 5)	.99	12.78	2.19	2.58	.61
	OVERRUN	0	--	--	--	--
A2	DEF. WIN (RATIO ≤ 1)	.70	5.17	5.17	1.08	1.08
	ATT. WIN (RATIO ≥ 5)	.28	8.64	1.46	2.97	.63
	OVERRUN	.02	3.0	2.0	1.0	1.0
A3	DEF. WIN (RATIO ≤ 1)	.22	4.73	4.73	1.29	1.29
	ATT. WIN (RATIO ≥ 5)	.76	10.64	1.72	3.11	.74
	OVERRUN	.02	3.50	2.50	.50	.50
A4	DEF. WIN (RATIO ≤ 1)	.39	3.79	3.79	1.52	1.52
	ATT. WIN (RATIO ≥ 5)	.57	9.11	1.44	3.41	..70
	OVERRUN	.04	3.25	1.25	.43	.43
A5	DEF. WIN (RATIO ≤ 1)	.64	4.37	4.37	1.42	1.42
	ATT. WIN (RATIO ≥ 5)	.33	9.24	1.52	3.52	.78
	OVERRUN	.03	3.0	1.0	.82	0

FIGURE 5: RESULTS OF COMBAT IN SCENARIOS A1 THROUGH A5 USING SIMULATION VERSION 1

These two studies, taken together, indicate that present methods for classifying scenarios as similar do not result in scenarios which are actually similar with respect to the prediction of combat results and suggest the need for the design of improved measures of terrain line-of-sight behavior. It is from this point that the research project described in this report commenced.

PART I, CHAPTER 2: RESEARCH PROBLEM

Past methods of classifying tactical exposure histories (scenarios) as similar have had the property that similar forces fighting simulated battles in similar scenarios would have widely diverse combat results. The research problem for this project was the identification of measures of tactical exposure which would cure this problem.

Past research [Farrell and Freedman, 1975] had indicated that the probabilities of win in "similar" scenarios as then defined had a standard deviation due to suppressed scenario differences of about 20 to 30%, and were therefore predictable to within only $\pm$ 40 to 60%. Other measures of combat results all showed similar, unacceptably large variations, all of which were caused in large part by the variability in probabilities of win. The goal of the current research was to identify measures of exposure similarity which would halve the residual variance (reducing the standard deviation by 30%) so that probabilities of win on similar scenarios would vary by no more than about 15% (standard deviation). It was felt that this level of residual variability would generally contribute no more variation in the results of Army studies than that contributed by the residual errors of estimation caused by the limited numbers of Monte-Carlo replications typical of such studies.

In addition to this basic goal, it was desired that the new method for classifying terrain exposure scenarios as similar would be usable on existing terrain data, rather than requiring new measurements to be taken in order for it to be used. It was also hoped that the new measures

might permit some separation of the contributions of terrain itself (including landform and vegetation) from those of the tactical use of terrain.

Specifically, this project set out to identify a set of measures M_i of exposure scenarios such that:

(1) the standard deviation of the probability of win for identical forces fighting simulated combats on different scenarios with identical measures M_i is no greater than 15 to 20% (with comparably improved accuracy on other combat result measures),

(2) the measures M_i could be determined for such already-measured terrains as those of the TETAM study by using existing terrain line-of-sight data without new field measurements,

and

(3) straightforward procedures can be used to estimate the separate contributions of terrain and of the tactical use of terrain to the overall measures M_i of the exposure histories.

Chapter 3 describes the research design used to address this problem, including the identification of the initial set of measures for testing and the conceptual strategy designed for adaptive modification of these measures if the initial set did not offer sufficient improvement in the variance results. Chapter 4 describes the results of the research (which was successful). It includes definitions of the successful measures M_i, statistical estimates of the variability of the probability of win for simulated battles on terrains with identical M_i, a discussion of the limitations of the measures, a description of methods for using existing data to estimate the M_i, and a discussion of methods for separating the contributions of attacker tactics from those of terrain and defender location. Chapter 5 summarizes the major conclusions derived from these results.

PART I, CHAPTER 3: RESEARCH DESIGN

In order to search for measures of tactical exposure which would
classify scenarios in the sense described in chapter 2, it was necessary
to make preliminary decisions on several areas of research designs. A
method had to be chosen for generating exposure scenarios to be used in
testing the validity of various classifications. A combat model or models
had to be selected with which to generate the combat results for each
scenario. The quantitative measures of combat results to be used had to be
specified. An initial class of terrain measures for study had to be selected,
and a conceptual method was required for the identification of additional
measures, should the initial candidates fail to satisfy the study criteria.
This chapter discusses the research design decisions made in each of these
areas.

3.1 Exposure Scenarios

Several alternative possibilities were available in the design of
scenarios for the study. Existing scenarios from actual Army weapon system
and force design studies could be used. New scenarios could be generated
using map exercise and computer-aided exposure analysis techniques. This
study could use statistical techniques similar to those used in the first
study of exposure-induced variability of combat results (see chapter 1 and
[VRI, 1970]. Such statistical techniques could be used, not to generate
scenarios *de novo*, but to provide a statistical population in addition to
and based on an existing set of study scenarios. The scenarios from the
1973-74 study could be used (see chapter 1 and [Farrell and Freedman, 1975]).

It was initially felt that the use of actual weapon system study scenarios would be most appropriate for this research. These scenarios have generally had a great deal of military analysis go into their initial design, and have evolved into more and more satisfying forms as they have been exposed to -- and improved after -- generations of criticism. However, the project team could not identify more than five such scenarios which were available and contained sufficient information on detailed exposure histories for use in this research. No more than three of these could be used, without modification, with a single Monte-Carlo combat model (although one deterministic combat model could be used with all five). Five scenarios was clearly an insufficient statistical base for this study. There were other detailed reasons why some of them were not suitable for this project.

New scenario generation by map exercise techniques, supported by computer-aided exposure analysis, was felt to be inappropriate for this research because of the lack of sufficient expertise on the study team to produce scenarios which would be reasonably representative. New scenario generation by statistical means suffered from the same potential problems -- its possible tactical unreality -- that had led to the 1973 extension of the original 1970, statistically based, work. Statistical extension of the five available weapon system study scenarios would not ensure any broader actual coverage than the five scenarios themselves.

The process of elimination of the alternatives outlined above left one -- the use of the 14 exposure scenarios from the 1973-74 study.

These were still available. They had been significantly criticized
and revised in that study, and appeared to offer a reasonable base for
the present research. The number of scenarios was enough that some
hope could be held out for statistically significant conclusions. The
project team considered using both these and some or all of the actual
weapon system study scenarios, but no combat model was available which
could be used with both kinds of scenarios without major efforts
being called for in either data transformation or model modification.
The final choice was to use the 14 1973-74 scenarios, supplemented
wherever useful by a statistically generated population similar to
themselves.

3.2 *Combat Models*

This choice of scenarios somewhat reduced the problem of choosing
a combat model. Because the scenarios included smoke and more ex-
posure detail than most models can treat, only three combat models
were potential choices. One of these was a deterministic model and
was eliminated on that grounds. The other two, differing in the
details of the submodels involving acquisition and suppression, had
both been used in the 1975 study. It was decided, as in that study,
to use both models in this study also, although making measurements
of combat result variability only for a single model at a time, to
avoid compounding model-induced variability with exposure-induced
variability.

The models used are fully documented in appendix A of [Farrell and Freedman, 1975] and only a brief description will be given here. The models are based on the rate-based (Markov) Monte-Carlo simulation methodology, previously exemplified by COMAN [Clark, 1969] (of which a variant called COMANEX is in use at General Research Corporation (GRC)) and an unnamed model by Andrighetti [1973]. These models have been shown in recent work by Clark and GRC to portray combat dynamics essentially identical to those of more complex Monte-Carlo simulations, and any minor differences appeared to be unimportant for the purposes of this study. The overall computational structure adopted, as shown in figure 6, is comprised of four (4) processors: the attrition rate processor, the scenario processor, the simulation processor, and the statistical processor.

The function of the attrition rate processor is to calculate the individual attrition and firing rates for each of the weapon systems under consideration. Input data for this processor consist of detailed weapon system performance data (e.g., rates of fire, hit probabilities, muzzle velocity, etc.). The output of this processor is stored on magnetic tape for later use by the scenario processor. Both models share a common attrition rate processor.

```
┌──────────────────┐                              ┌──────────────┐
│ Weapon Systems   │                              │   Scenario   │
│     Data         │                              │     Data     │
└──────────────────┘                              └──────────────┘
        │                                                 │
        ▼                                                 ▼
┌──────────────────┐      ╭──────────╮          ┌──────────────┐
│ Attrition Rate   │─────▶│ Attrition│─────────▶│   Scenario   │
│   Processor      │      │   Rate   │          │   Processor  │
└──────────────────┘      │   Tape   │          └──────────────┘
                          ╰──────────╯                  │
                               │                        ▼
                               ▼                   ╭──────────╮
                     ┌──────────────┐              │ Scenario │
                     │  (Optional)  │─────────────▶│   Tape   │
                     │ Statistical  │              ╰──────────╯
                     │  Scenario    │                   │
                     │ Generation   │                   ▼
                     └──────────────┘          ┌──────────────┐   ┌──────────────┐
                                                │  Simulation  │◀──│  Simulation  │
                                                │   Processor  │   │     Data     │
                                                └──────────────┘   └──────────────┘
                                                       │
                                                       ▼
                                                 ╭──────────╮
                                                 │ Casualty │
                                                 │ History  │
                                                 │   Tape   │
                                                 ╰──────────╯
                                                       │
                                                       ▼
                                                ┌──────────────┐
                                                │ Statistical  │
                                                │  Processor   │
                                                └──────────────┘
                                                       │
                                                       ▼
                                                ┌──────────────┐
                                                │ Statistical  │
                                                │   Reports    │
                                                └──────────────┘
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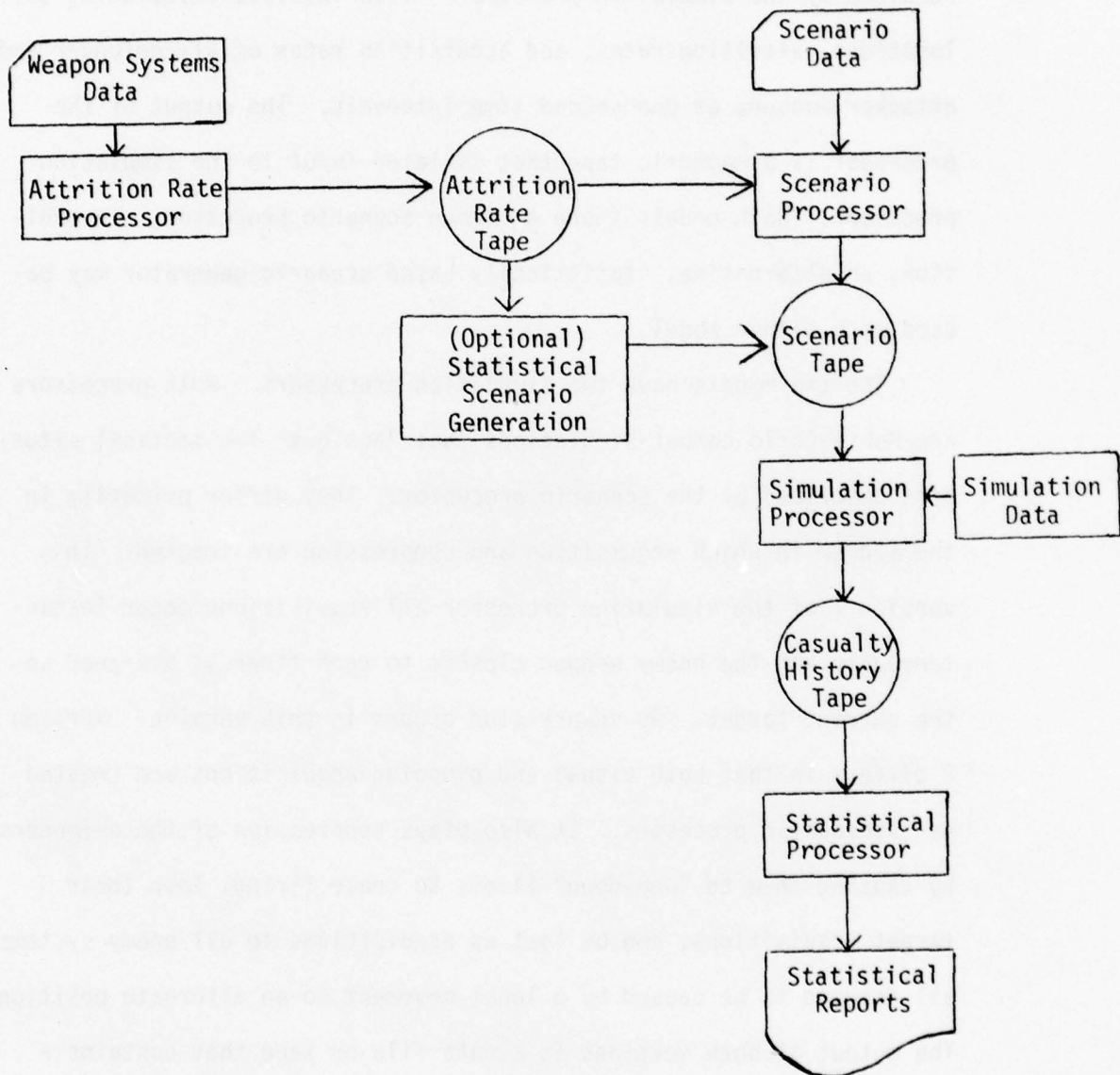
FIGURE 6: OVERALL STRUCTURE

The function of the scenario processor is to convert the tactical scenario description data originally supplied by AMSAA into the format required by the simulation processor. This involves calculating the locations, attrition rates, and acquisition rates of all defender and attacker weapons at one-second time intervals. The output of the processor is a magnetic tape that is later input to the simulation processor. Both models share a common scenario processor. In addition, an alternative, statistically based scenario generator may be used with either model.

The two models have two simulation processors. Both processors are Monte-Carlo combat simulations that "act out" the tactical situation described by the scenario processor. They differ primarily in the manner in which acquisition and suppression are treated. In version l of the simulation processor all acquisitions occur instantaneously and the enemy weapon closest to each firer is assigned as the current target. No suppression occurs in this version. Version 2 differs in that both visual and pinpoint acquisitions are treated as stochastic processes. It also plays suppression of the defenders by causing them to "pop-down" (i.e., to cease firing, lose their target acquisitions, and be lost as acquisitions to all enemy systems -- all assumed to be caused by a local movement to an alternate position). The output of both versions is a data file or tape that contains a chronological history of when casualties occurred during each replication of the battle. This casualty history is later input to the statistical processor.

The function of the statistical processor is to produce statistical reports that summarize the casualty history data produced by the simulation processor. The basic output of this processor is a time history of each battle. This history includes. (a) the mean number of survivors, (b) the standard deviation of the number of survivors, and (c) the probabilities that each side is annihilated. Since all casualty data is stored on the casualty history tape, it is possible to produce any type of statistical report without re-running the simulation processor.

3.3 *Combat Result Measures*

As with any combat analysis, a wide variety of possible combat result measures were potentially usable in this research. These included probabilities of defender (or attacker) win for various win criteria, surviving weapons, losses, exchange ratios, and other measures. A preliminary analysis of these measures was run, using the exposure scenarios and combat forces (and therefore results) from the 1973-74 study. This analysis indicated that the conditional variability in survivors, losses, or exchange ratios, given an attacker win (or a defender win), was less than one-fourth of the total variability. That is, the variability of the probability of win explained 75% of the variability of the other measures. Accordingly, the probability of defender win (PDFWIN) was selected as the principal combat result measure whose variability was to be reduced.

3.4 Exposure Classification Measures

The choice of candidate measures of exposure for testing was considered critical to the success of this project. In order to give the project the greatest possible *a priori* probability of success, a broad group of approaches was taken to the problem of designing terrain measures.

First, several statistics which have been historically used to quantify the degree of tactical exposure in a situation were selected, so that any good classification schemes which could be developed based on past work would be discovered and used. Since earlier work had made clear that many of the past measures would not perform adequately in classifying exposure scenarios consistently with the predicted combat results in them, slightly modified forms were also generated in order to retain the greatest possible consistency with prior work.

A second approach which was taken was to examine the logic of representative combat models and derive mathematical approximations to the effects of exposure on attrition in these models. The combat models which were selected for use in this research project were not used in these mathematical pre-analyses, in order to take no chance of designing exposure measures around any model-specific features in its treatment of exposure.

The third approach -- which generated the largest class of alternative measures, but also the least simple -- was to identify the

most detailed class of exposure data, short of complete, weapon-by-weapon exposure traces, which were available from field experimental measurements such as those of TETAM, and to generate a wide class of parametric functions of these data as candidate measures. Summaries of the results of each of the three approaches are given in individual sections (3.4.1, 3.4.2, and 3.4.3) below.

3.4.1 Exposure Measures Selected Based on Prior Use

Earlier studies (see chapter 1 and [VRI, 1970] and [Farrell and Freedman, 1975]) had shown that predicted combat results showed no correlation with the average exposure length in (in meters) of an attacker, the average fraction of a random attacker path exposed to a random defender,[1] or the average number of simultaneous exposures in a scenario. These results held even when range-interval dependent versions of these measures were examined. These measures had been used directly on classification measures in the 1970 work and were also used as control checks on the subjective classifications of the 1973-74 study. Accordingly, no direct work with them was undertaken in this research.

However, no past work had been done on the variability question with opening range or last covered range statistics. Accordingly, both the opening range and the last covered range -- defined as the maximum range below which attackers were 90% exposed -- were considered as candidate measures. On the 14 scenarios involved, 75% of the attrition fell *outside*

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[1]A statistic often called PLOS. As will be discussed below, the symbol PLOS was used for a variant of this measure in this research project.

covered range, however, and this measure was therefore dropped from consideration as not explaining the high variability.

In addition to the opening range, a time-oriented variation of the more normally range-oriented average exposure measure, PLOS, was adopted as a candidate. This measure was defined as the average fraction of an attacker time-line exposed to a random defender.[1] The use of duration, rather than path-length, as the basis of measurement allowed for explicit treatment of planned overwatch fires and similar periods in which some portion of an attacker force was stopped in a firing position. The average PLOS over an entire scenario was one candidate measure. A second was the PLOS of a range interval from the opening range, O.R., to O.R. - 1000 m. (This scenario-dependent 1000 m. range interval will be referred to as the opening range interval).

3.4.2 Exposure Measures Suggested by Combat Model Analysis

Mathematical analysis of the DYNTACS model was conducted in order to obtain approximate formulae relating exposure to attrition. The DYNTACS model [Clark, 1971][2] was chosen on two bases: it is one of the five most detailed high-resolution combat models in use in the United States, and it was the least similar of these five to the

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[1]Using time-line durations, rather than path lengths, as the basis of the measure distinguishes this use of the PLOS notation from that used by CACDA and CDEC in presenting TETAM results.

[2]The mathematical analysis was conducted on the basis of the extant DYNTACS model documentation, even where it differed with current implementations of the model, in order that a consistent picture of the model could be obtained.

Monte-Carlo combat models adopted for this research, thus assuring the least possible chance of designing model idiosyncrasies into the candidate measure and then having the measure pass the research tests which used the Farrell-Freedman [1975] model).

The analysis, which is reported here only in summary, led to observations listed below. In the cases where the abstract mathematical analysis was confirmed by analysis of actual model results (as obtained from CACDA during the conduct of the CLGP and HELLFIRE COEA studies), this fact is noted.

> *Observation 1: Firing Determines Attrition.* It was
> observed that Blue losses were essentially directly
> proportional to Red firing and Red losses to Blue
> firing, with constants of proportionality determined
> from weapon system performance parameters. Although
> the exact relation shows range-dependent constants of
> proportionality, the mathematical analysis suggested
> that the effects of range dependence (and the various
> probabilistic variations which could be expected)
> on these relationships would be minor (of the order
> of 10% to 20%). This observation was confirmed from
> actual DYNTACS run outputs. Simply restating this
> observation in different form gives: *the ratio of*
> *actual Blue firing to actual Red firing determines*
> *the force exchange ratio in combat; the rates deter-*
> *mine the rate of attrition and eventually the*
> *duration of battle.*

Observation 2: Exchange Ratio Determines PWIN. The
mathematical analysis of DYNTACS suggested that, apart from
cases where force effectiveness was dominated by a single
weapon or two, there should be an extremely high correla-
tion between the probability of win (defined either in
terms of numbers of losses or of survivor ratios) and the
expected exchange ratio. Combined with observation 1,
this gives: *the ratio of actual Blue firing to actual Red
firing determines the probability of win in a battle.*

Observation 3: Tactics and Detection Limit Firing.
Pursuing the determinants of actual firing, it was clear
that firing in DYNTACS was negatively controlled: all
weapons would engage in continuous firing except when
(a) they were tactically constrained (out of effective
range, moving, etc.) or (b) they had not detected any
targets. There was some interaction between these, in
which the decision to move (or to stop) was governed
partly by the availability of targets, so that detections
seemed a slightly more powerful determinant. The inclu-
sion of tactical controls, however, suggested that any
final measure of exposure should treat periods of expo-
sure which were tactically suitable for firing
differently than those in which one or both of the

mutually exposed weapons would be tactically constrained not to fire.

Observation 4: Exposure Affects Detection. In tracing the relation between exposure and detection, it became clear that there were three determinants of detection in DYNTACS: exposure, survival (dead observers have no detections, and dead targets are not detected), and weapon system performance. Summarizing the relations in a simple approximate mathematical formulation, one obtains

$$P_D = P_{E,1} \cdot P_{S,1,1} \cdot P_{d,1}$$
$$+ P_{E,2} \cdot (P_{S,2,1} \cdot P_{d,1} + P_{S,2,2} (1 - P_{d,1})^2)$$
$$+ \cdots$$

where

P_D is the probability that a weapon tactically able to fire has a suitable detected target,

$P_{E,i}$ is the probability that such a weapon has i targets exposed to his view,

$P_{S,i,j}$ is the probability that of i such exposed targets, j have survived to this point in the battle, and

$P_{d,1}$ is the probability that such an exposed surviving target will have been detected.

Because this formulation included survivability and detection parameters which could not reasonably be included in a measure of exposure, the following even simpler parametric approximation to the true relation was projected:

$$P_D^{[N_0]} = k \sum_{i=N_0}^{\infty} P_{E,i} \quad ,$$

where

k is an unknown proportionality constant, and

N_0 can be varied parametrically among the small integers (say, 1, 2, 3) to provide alternative possible parametric approximations to the exact relation.

For a given N_0, $P_D^{[N_0]}$ is proportional to the average fraction of time for which a random weapon has at least N_0 targets exposed to view.

Putting these observations on the approximate structure of the DYNTACS model together, an exposure measure of the following form was suggested.

$$M_{N_0} = \frac{ALOS(N_0)}{DLOS(N_0)} \quad ,$$

where

ALOS(N_0) is the mean number of seconds (in a range band) in which an attacker weapon system would have N_0 or

more exposed targets and would be tactically
permitted to fire, and

$DLOS(N_0)$ is the mean number of seconds (in a range band) in
which a defender weapon system would have N_0 or
more exposed targets and would be tactically permitted
to fire.

The cases N_0 = 1, 2, or 3 are of particular interest. These candidate
measures are producible for the TETAM exposure scenarios from the data
measured in TETAM -- assuming constant average velocities (as has
typically been done in interpretations of the TETAM data) and making
some assumptions concerning the firing tactics and capabilities to fire
on the move of the attacker. Some further separation of terrain and
tactics seemed possible, and a discussion of this will be found in
chapter 4. Accordingly, these measures were taken as candidates for
examination. Initial testing was to be on ALOS(1)/DLOS(1) for the
opening range interval (defined in section 3.4.1).

3.4.3 A Broad Class of Parametric Measures

The most detailed class of available exposure data (short of complete
weapon-by-weapon exposure histories) is of the form $F_D(a,i)$, $F_A(d,i)$
where

$F_D(a,i)$ is the fraction of the defending weapons which
have at least a fraction   a   of the attacking
force exposed to their view at range (or time) i,
and

$F_A(d,i)$ is the fraction of the attacking weapons which
have at least a fraction d of the defending
force exposed to their view at range (or time) i.[1]

A very broad class of measures of exposure can be generated from the
linear rational functions of these, i.e.,

$$\frac{C_A + \sum_i \int_d F_A(d,i)\, d\mu_i(d)}{C_D + \sum_i \int_a F_D(a,i)\, d\eta_i(a)} .$$

In the event that no previous measure or group of measures of exposure
should prove adequate for classification of exposure scenarios, this
class of parametric measures was selected for investigation. (It should
be noted that ALOS(1)/DLOS(1) is one example of such measures with
$C_A = C_D = 0$, and μ_i and η_i atoms at 0.)

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[1] We consider $F_D(0,i)$ and $F_A(0,i)$ to be $\lim_{\varepsilon \to 0} F_D(\varepsilon,i)$ and $\lim_{\varepsilon \to 0} F_A(\varepsilon,i)$,
rather than identically 1.0. That is, $F_D(0,i)$ (or $F_A(0,i)$) is the
fraction of defending (or attacking) weapons with any of the attacking
(or defending) force exposed to them at range (or time) i.

PART I, CHAPTER 4:   RESEARCH RESULTS

This chapter is organized in three sections. The first reports on
the statistical estimation of the efficiency of candidate exposure classi-
fication methods and the resulting identification of a single promising
classification technique. The second summarizes sensitivity analyses of
the method, discusses the limitations of the method discovered in these
analyses, and describes possible extensions which may be used in cases where
the basic method is inapplicable. The third discusses the separation of
the effects of terrain and tactics and a resulting problem in the analysis
of certain details of tactical control and coordination of forces. The
major conclusions derived from these results are summarized in chapter 5.

4.1 *Statistical Analyses of Exposure Classification Measures*

As was discussed in chapter 3, the initial phase of this research was
directed to identifying measures of tactical exposure which would reduce
the conditional variability of the probability of (defender) win, PDFWIN,[1]
given knowledge of the exposure measure, to acceptable levels. Two Monte-
Carlo combat models, differing in their submodels of acquisition and sup-
pression, were used. A preliminary analysis was made of the degree of
agreement of the two models. Figure 7 displays the results, in terms of
PDFWIN, of a hundred replications of each model on each of the ten

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[1]Defender win occurs in these scenarios when the surviving force ratio has
 reached 1 to 1 (the initial ratio is 2.5 to 1; attacker win is a ratio of
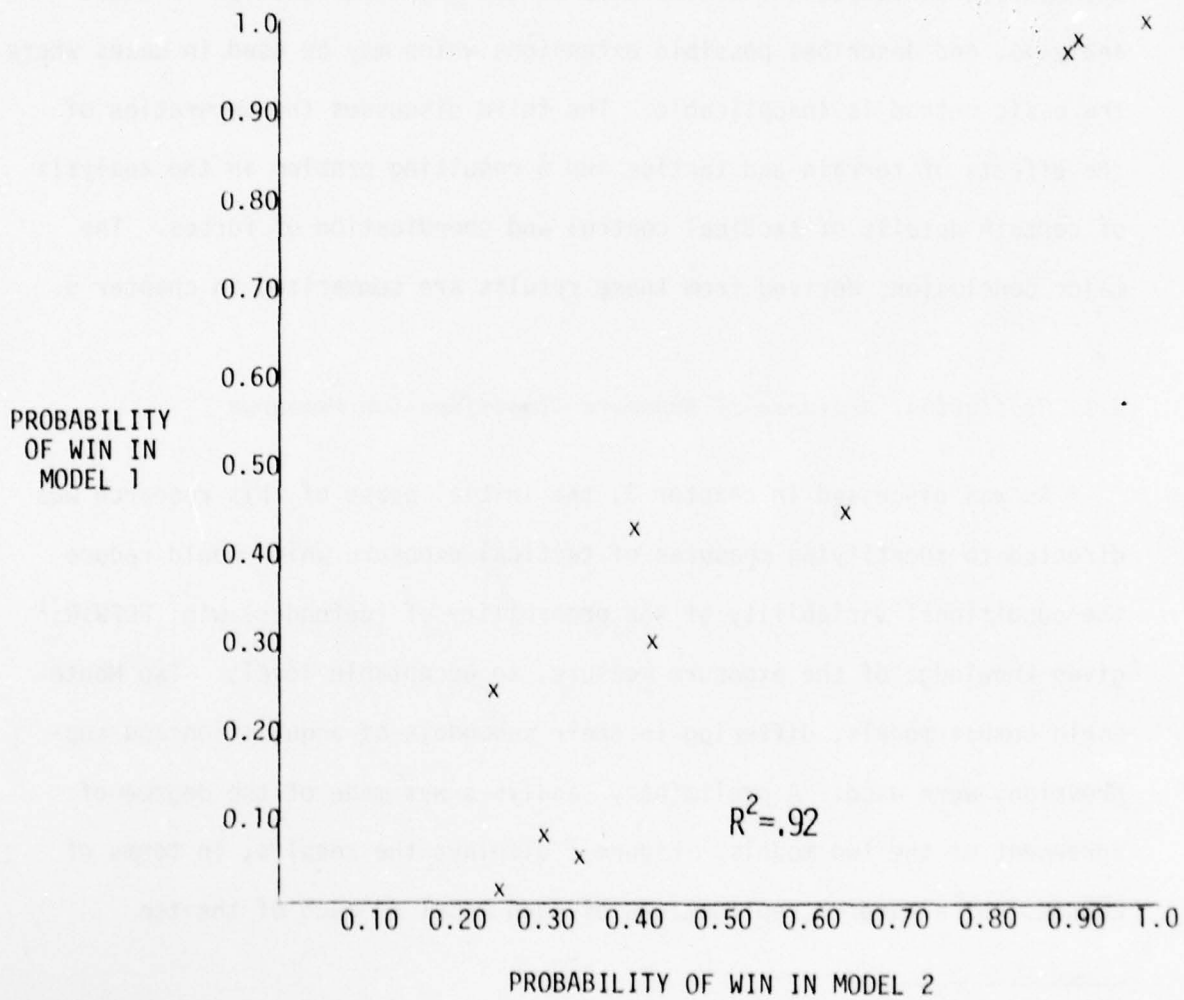 5 to 1).

FIGURE 7: CORRELATION OF PDFWIN IN MODEL 1 AND MODEL 2

scenarios suited to both models.[1] (Four of the 14 available scenarios were in formats usable only by model 1.) As is plain from figure 7, the difference between the models is not significant in comparison with the variability introduced by exposure scenarios. (In fact, the R^2 value of .92 for the squared correlation between PDFWIN (model 1) and PDFWIN (model 2) indicates that either model result explains 92% of the variation in the other.[2]) Because of this good agreement between the models, the remaining graphical displays in this section will involve only model 1, although all analyses were performed on both models.

The first exposure statistic examined for possible association with predicted combat results in this study was the opening range, defined as

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[1]Chapter 3 discusses the selection of the basic scenarios.

[2]R^2 is defined as

$$\frac{\left[\sum_i (x_i - \bar{x})(y_i - \bar{y}) \right]^2}{\sum_i (x_i - \bar{x})^2 \sum_i (y_i - \bar{y})^2}$$

for a set of data points (x_i, y_i). It is a standard statistical variable measuring the fraction of the total variance in some variable which is explained by variations in another variable. Similar formulae exist with the same meaning for cases where variation in a single variable z_i is being analyzed in terms of several explanatory variables $(u_i, v_i, w_i, \ldots)$.

A variance explanation of R^2 corresponds to a reduction in the standard deviation of the explained variable by a factor $\sqrt{1-R^2}$, so that an explanation of 50% of the variance reduces the standard deviation by 30%, and an explanation of 25% of the variance reduces the standard deviation by 13%.

the maximum range at which any attacker-defender exposure occurred. A plot of this opening range versus PDFWIN for 100-replication samples of 14 exposure scenarios is shown in figure 8. As is apparent from the graph, opening range does not serve as a good predictor of PDFWIN in these scenarios. As is indicated by the R^2 value of .29, opening range explains only 29% of the variation in win probability. In fact, the R^2 value for only those cases with opening ranges under 2500 meters is only .07, which indicates that what little explanatory power opening range does have is due almost entirely to its association with the two cases in which the opening range is significantly beyond the range of the attacker weapons. The results for model 2 are essentially identical. In interpreting these R^2 values, it should be noted that R^2 values less than .23 are statistically not distinguishable from 0 at the 5% level, and all R^2 values less than .37 are statistically not distinguishable from 0 at the 1% level.[1]

The statistic PLOS, defined in this research as the average fraction of the time a random attacker was mutually intervisible with a random defender in the exposure scenario,[2] was examined next. Figure 9 shows the correlation between PLOS and PDFWIN. There is no significant association. The R^2 value of .0036 is not significant. A modified version of PLOS,

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[1]On our 14 point samples.

[2]It should be noted that, in the company-scale actions in our scenarios, essentially all exposures were within weapon range, so that alternate versions of PLOS taking range coverage into account showed no differences from this definition.
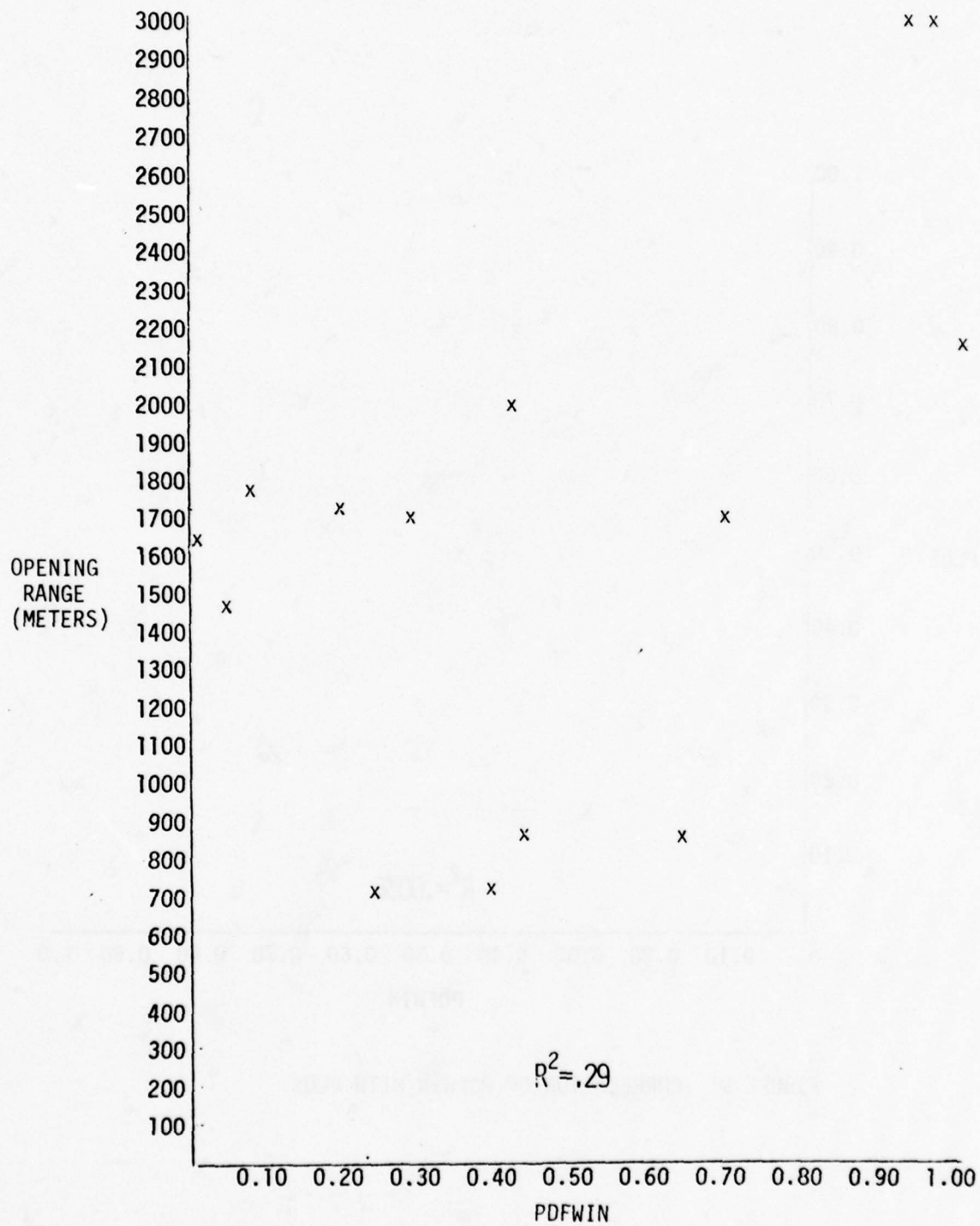
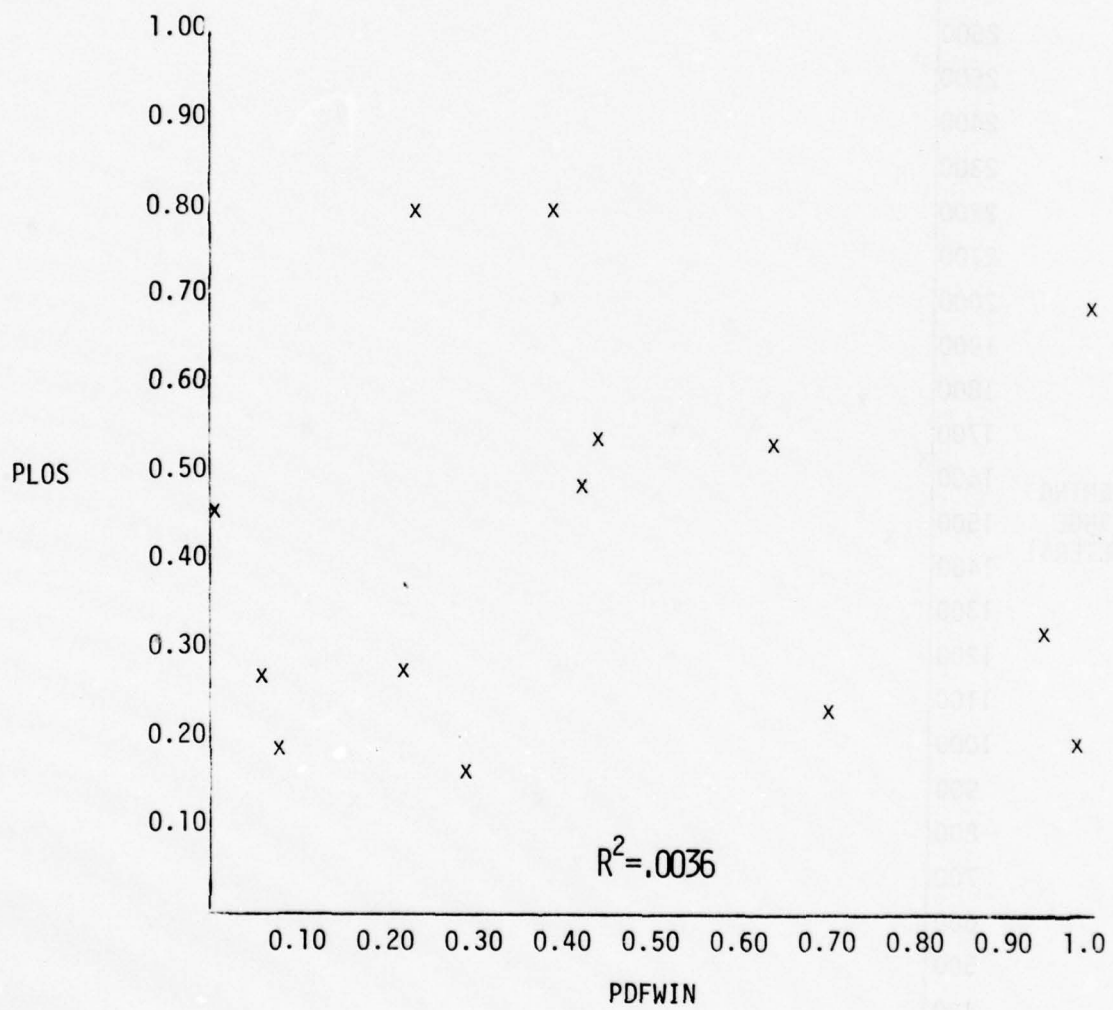FIGURE 8: CORRELATION OF PDFWIN WITH OPENING RANGE

FIGURE 9:   CORRELATION OF PDFWIN WITH PLOS

emphasizing the portions of the battlefield where attrition actually takes place, is PLOS restricted to the opening range interval, defined for this research as a range interval of 1,000 meters beginning at the opening range. Figure 10 plots this modified PLOS statistic against PDFWIN. Again, as is apparent on inspection, there is no statistically significant association between PLOS in the opening range band and PDFWIN. The R^2 value of .11 would be statistically distinguishable from 0 at about the 13% level. Even if there is a non-zero correlation, it is not of the order of magnitude which was sought in this study.

As was discussed in chapter 3, the research design for this study also called for examination of the statistic ALOS(1)/DLOS(1) in the opening range interval, where

$\text{ALOS}(N_0)$ = mean number of seconds (in a range band) in which an attacker weapon system would have N_0 or more exposed targets and would be tactically permitted to fire, and

$\text{DLOS}(N_0)$ = mean number of seconds (in a range band) in which a defender weapon would have N_0 or more exposed targets and would be tactically permitted to fire.[1]

Figure 11 plots this statistic against PDFWIN for 100-replication samples of our 14 exposure scenarios.[2] As is clear on inspection, this measure provides

[1]The derivation of this candidate measure was described in some depth in chapter 3.

[2]Figure 12 contains a table of the data plotted on figures 8 through 11, for reference. It also contains rank order data for each statistic.

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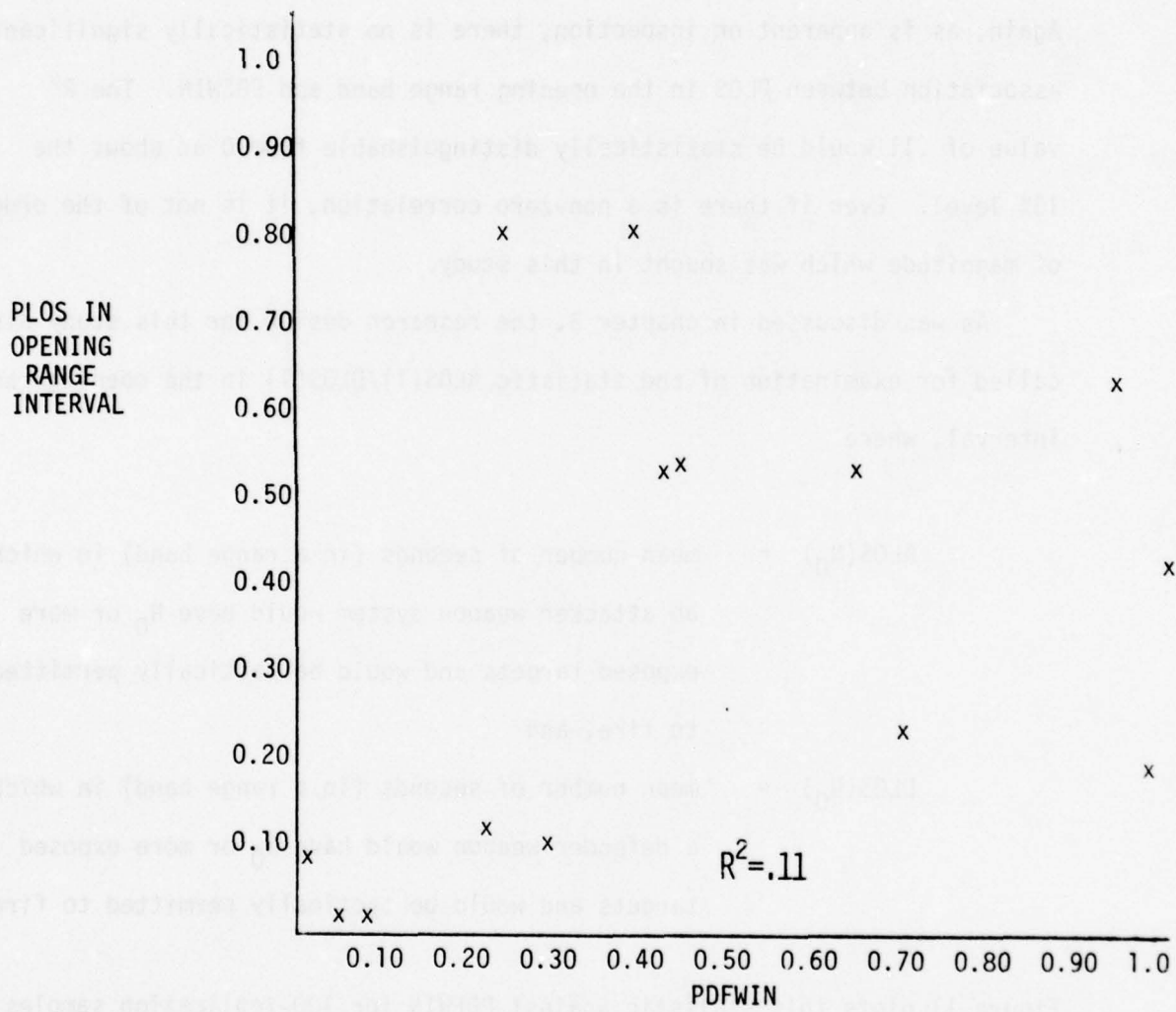FIGURE 10: CORRELATION OF PDFWIN WITH PLOS IN
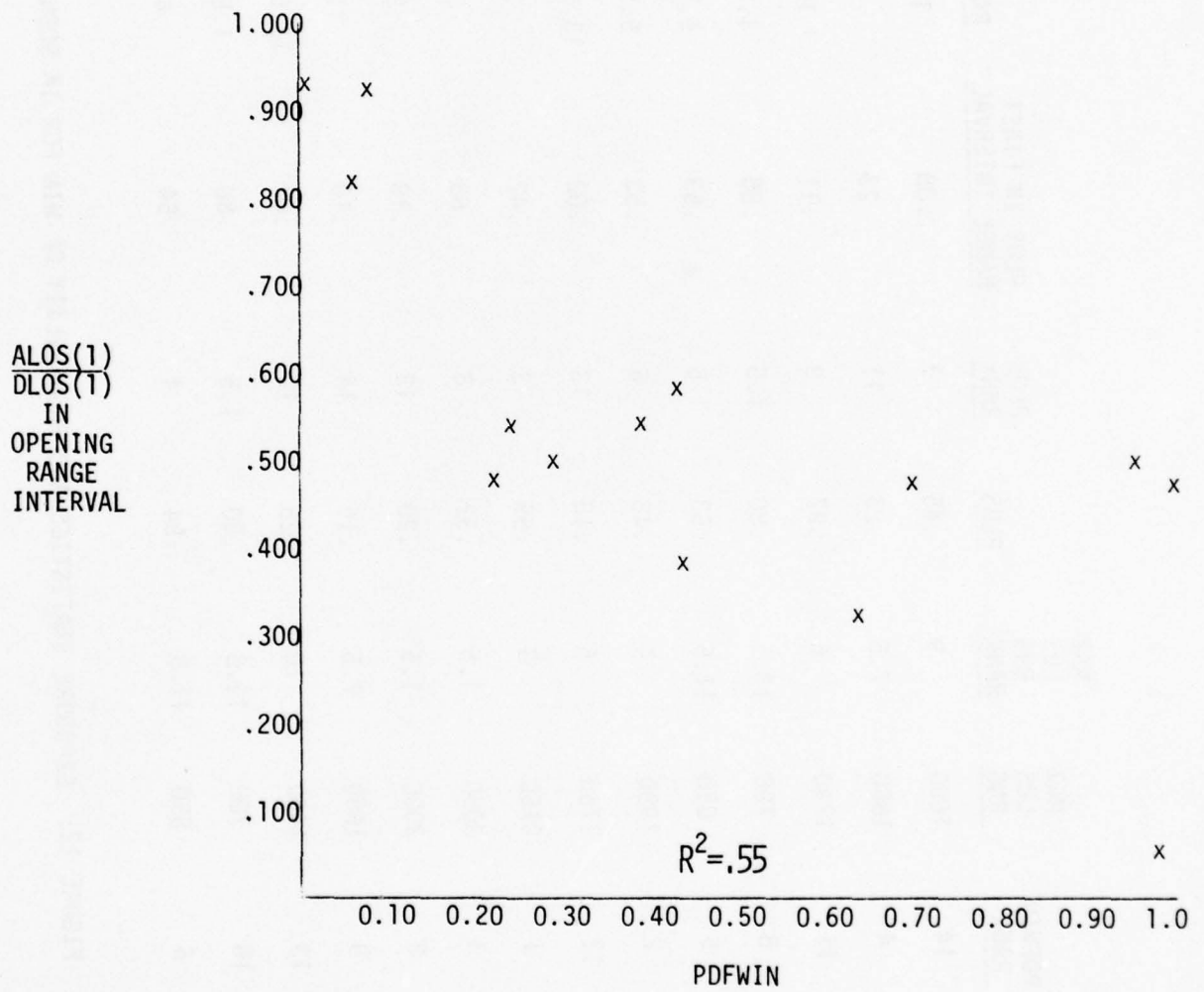OPENING RANGE INTERVAL

FIGURE 11:   CORRELATION OF PDFWIN WITH ALOS(1)/DLOS(1)
IN OPENING RANGE INTERVAL

SCENARIO	PDFWIN	PDFWIN RANK	MAX LOS RNG	MAX LOS RNG RANK	PLOS	PLOS RANK	PLOS IN FIRST RANGE INTERVAL	RANK	ALOS/DLOS IN FIRST RANGE INTERVAL	ALOS/DLOS RANK
A1	.01	14	1650	9	.45	7	.08	12	.933	14
A2	.70	4	1680	7.5	.23	11	.23	8	.488	6
A3	.22	11	1730	6	.27	9	.11	10	.484	5
A4	.39	8	700	13.5	.80	1.5	.80	1.5	.550	10
A5	.64	5	850	11.5	.53	5	.53	5.5	.320	2
B1	.42	7	1980	4	.48	6	.53	5.5	.580	11
B2	.08	12	1760	5	.18	13	.02	13.5	.930	13
B3	1.0	1	2150	3	.69	3	.42	7	.471	4
B4	.95	3	3000	1.5	.32	8	.63	3	.492	7
B5	.98	2	3000	1.5	.20	12	.18	9	.157	1
C2	.29	9	1680	7.5	.16	14	.10	11	.495	8
C3	.05	13	1460	10	.26	10	.02	13.5	.314	12
C4	.24	10	700	13.5	.80	1.5	.80	1.5	.530	9
C5	.44	6	850	11.5	.54	4	.54	4	.380	3

FIGURE 12: EXPOSURE STATISTICS AND PROBABILITY OF WIN FOR 14 SCENARIOS

a significant association with PDFWIN. In fact, the R^2 of .55 is statistically distinguishable from 0 at the 0.25% level. If a single additional indicator -- whether the opening range is greater than the attacker weapon range -- is used to provide a slightly finer classification, the R^2 value becomes .65, and is statistically significant at the 0.05% level. The model-2 R^2 (without the fine classification by opening range distinctions) is .52 (on a sample of ten scenarios) and is also significant at the 1% level.

While the use of the R^2 statistic to measure association would normally imply a continuous and approximately linear relationship between variables was expected, no such implication is intended here. With the currently limited evidence, the only conclusion that can be reached is that ALOS/DLOS strongly influences the battle probability of win. The relationship between these variables obviously does not account for all of the scenario effects on the win probability, and the effects which it does explain may be highly non-linear. Further, the proportion of scenario effects which it does explain may vary in different regions of ALOS/DLOS.

A hint of such variation in the variability in the quantitative tightness of the relation between ALOS/DLOS and the win probability exists in the current data. At the more extreme ends of ALOS/DLOS values, the prediction is quite good, while for more moderate values of ALOS/DLOS there appears to be a wider remaining variability caused by other effects. Any analysis of this effect will, however, require additional research with larger data sets. In any case, the capability of the ALOS/DLOS statistic to allow the identification of the extremes--that is, the stratification of the games into even the three categories of clear defender wins, clear attacker wins, and cases

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in which other scenario details will influence the probability of win--is a major step forward in the production of operationally significant measures of line of sight.

Since the target for variance reduction was approximately 50%, and this has been achieved by the ALOS(1)/DLOS(1) statistic in the opening range band, the basic exposure measurement problem appears to have at least a partial solution.[1] The next section will discuss sensitivity analyses performed on this measure in an attempt to identify its limitations.

4.2 *Sensitivity Analyses*

Sensitivity analyses of the association of ALOS(1)/DLOS(1) in the opening range band with PDFWIN were performed to identify sensitivities with respect to

(1) the exact sample exposure scenario used,

(2) the exposure statistics of the scenarios used,

(3) the details of the definition of the measure, and

(4) the weapon tactics and performance data and assumptions used in the combat simulation.[2]

In no case was the intent of these analyses to improve the performance of the ALOS(1)/DLOS(1) exposure classification measure,[3] but rather to identify potential areas where its classificatory performance would suffer, and, if possible, to suggest appropriate treatments for such cases.

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[1]This finding is borne out by the fact that the association between ALOS(1)/DLOS(1) in the opening range band and the loss ratio (defender losses divided by attacker losses) is even stronger, with an R^2 of .68.

[2]As well as the model 1 versus model 2 sensitivity reported on in the earlier section.

[3]See chapter 3 for a description of the reasoning behind the selected target performance.

The first sensitivity analysis involved the statistical generation of 100 exposure scenarios with exposure duration and total exposure statistics similar to those of the original fourteen scenarios.[1] Twenty-five replication samples of the combat model (combat model 1) were run on each scenario, and ALOS(1)/DLOS(1) for the opening range interval was computed for each. The resulting statistics had an R^2 of .52, indicating that the particular selection of scenarios was not a determinant of the effectiveness of ALOS(1)/DLOS(1) at predicting PDFWIN.

A similar sensitivity analysis was then conducted with three additional sets of 25 statistically generated scenarios in which sensitivity to average total exposure and average exposure duration ("window length") was tested. In one set, average exposure duration was reduced 25%, in one average total exposure was reduced by 25%, and in one both were reduced by 25%. The R^2 for ALOS(1)/DLOS(1) on the composite set including all three of these sets and the base set was .53. This indicated that -- within reasonably small variations -- the explanatory power of ALOS(1)/DLOS(1) in explaining variations of combat results with exposure scenario is not affected by changes in exposure characteristics.[2]

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[1] Chapter 3 discusses the selection of the basic scenarios.

[2] This conclusion is obviously invalid in the extremes, as can be observed by strictly mathematical sensitivity analyses. If the mean window length approaches 0 while ALOS/DLOS remains constant, the combat clearly changes significantly. All the cases used in this research involved mean durations of exposure (including stopped as well as moving times) which were greater than 60 seconds. Use of the ALOS/DLOS statistic as a single classifier for scenarios with mean exposure durations significantly less than this (say, 30 seconds or below) should be viewed as questionable. Similar mathematical sensitivity analyses indicate that cases in which some sections of an attacking force have significantly less total exposure than others may not group well with cases with more even exposure. This effect is insignificant unless the standard deviation of the amount of exposure of individual attackers is more than 1.5 times the mean exposure (a condition we have found in none of our scenarios).

Two kinds of sensitivity analyses were conducted with respect to the definition of ALOS(1)/DLOS(1) in the opening range band. First, the degree to which the 1,000 meter length of the opening range band could be changed was examined. The R^2 value for ALOS(1)/DLOS(1) on 100 scenarios as the range band changed was as follows:

Range Band	R^2
1,500 m.	.40
1,250 m.	.49
1,000 m.	.52
750 m.	.54
500 m.	.51

These results indicate that the explanatory power of the statistic is not affected by changes in the definition of the range band size from 500 m. to 1,250 m., but may decrease for longer bands. (The difference between .40 and .52 is not statistically significant at the 5% level, but would be at the 10% level.) The use of ALOS(1)/DLOS(1) rather than ALOS(2)/DLOS(2) or ALOS(3)/DLOS(3) was also examined. As with the range band size, no statistically significant differences were observed between the three measures. (The R^2 values were .52, .54, and .55, respectively.) Accordingly, this research cannot be said to have provided any basis for selection among the several possible ALOS/DLOS statistics as exposure classifiers.

In order to avoid potential dependencies of the exposure classification statistic on precise force sizes involved, it seems reasonable to adopt a definition of ALOS/DLOS with

ALOS =   mean number of seconds in the opening 1,000 meter range band in which an attacker weapon system would have 10% or more of the defender weapons exposed in areas in which it was tactically permitted to fire, and

DLOS =   mean number of seconds in the opening 1,000 meter range band in which a defender weapon system would have 10% or more of the attacker weapons exposed in areas in which it was tactically permitted to fire.

(On the basis of this study, any fraction from 1% to about 30% could reasonably be used. The choice of 10% is arbitrary, as is the use of the 1,000 meter band length.) This general form, which is equivalent to the definition used for all the research done in this study, seems to be slightly preferable as a general form for two reasons: it seems to avoid the risk of a sensitivity to force size, and it is easier to analyze the separate effects of terrain and tactics in this definitional form. (This latter area is discussed in section 4.3.)

Additional sensitivity analyses were run with respect to two important weapon system data items which seemed likely to interact with ALOS/DLOS in explaining results: one involved changes in acquisition parameters (and the use of model 2 versus model 1). The results were reported in section 4.1, and showed no significant reduction in the explanatory power of ALOS/DLOS for model 2 (and moderate acquisition rates) as compared to model 1 (and instantaneous acquisitions). A second sensitivity analysis involved the use of local defender movement (through covered areas to alternate firing positions). This sensitivity analysis involved 100 replications of the combat model on each of five scenarios, and produced an R^2 of .83. (The difference from .50 is not statistically significant on the small number of scenarios.)

4.3 *Analyzing the Separate Effects of Terrain and Tactics*

The examination of the separate effects of terrain and tactics in this project was conducted as a three stage effort. First simple analytic models of the ways in which terrain and tactics interact to produce ALOS/DLOS were sought. These were then analyzed to suggest possible hypotheses for experimental investigation, and finally experimental investigation was conducted to confirm the hypothesis which had been generated.

In order to describe briefly how the hypothesis that was finally experimented with was derived, the analytic model that was designed

for this research will be presented here without any derivation of the mathematical forms involved. None of the conclusions or results of this study were derived from this model; it served only as a method of focusing the investigations on particular topics. For this reason, the considerations which led to its exact mathematical form are not necessary for understanding the results of this project.

The model related the effects of three types of attacker tactical controls to the resulting ALOS/DLOS exposure statistic.[1] Specifically it assumed that

$$\frac{ALOS}{DLOS} = \frac{(1-h)\ mp_1 T + f}{(1-h)\ (1-(1-p_2)^c)T + f}$$

where

h = the degree to which the attacker manages to use covered routes of advance

p_1 = the fraction of the opening range band in which a random point in the area (1,000 m. deep and as wide as the defender front) is within tactical range of and has line-of-sight to at least 10% of the defenders

m = the fraction of the time an attacker is able to spend firing while advancing

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[1] No methods for separating defender deployment tactics from terrain parameters were discovered in this project.

T = the time required for an attacker to advance 1,000 m.

f = the average amount of time an attacker spends stopped in a firing position

p_2 = the average fraction of the opening range band area visible to a random defender

c = the attacker's degree of exposure coordination, a number between 1 and the number of attackers representing the average number of *independent* targets provided by the attacker

When sensitivity analyses of this model were run with what were felt to be reasonable ranges of data, the greatest sensitivity observed was always to the parameter c, representing the details of the control and coordination of attacker exposure. This suggested the hypothesis that the effects of attacker maneuver control tactics dominate the effects of terrain. That is, this high sensitivity to the details of exposure control and coordination led to an examination of the possibility that this area alone was the major source of variability on predicted combat results which this study had set out to analyze. In order to examine this possibility, a sample of six scenarios from the 14 human-generated scenarios used in this study was selected at random after eliminating the two scenarios in which the opening ranges were beyond the attacker weapon range. In each of these cases, an investigator then used a computer interactively to adjust the combat movement and observe the effects, adjusting only the time trace, and never the routes selected or the exposures at given position. In order not to violate system performance

limits, no movement speed was ever increased. In every case it proved possible by one set of such adjustments to force the probabilities of win to less than 17% and by another set of such adjustments to force them to more than 83%. (Thirty replications of each adjusted scenario were run, and sets with 5 or fewer wins or 25 or more were accepted.) It required an average of approximately 3 to 3.5 interactive adjustments to achieve both cases. All variations were deliberately designed to increase or decrease ALOS/DLOS as was appropriate to the variation being made. That such variations did manage to accomplish the expected changes further confirms the importance of the ALOS/DLOS statistic in predicting combat results, as well as offering new information.

This experiment confirmed that the variation of tactical details of movement coordination, without variation in terrain or in route selection, can cause variations in predicted combat results of the order of magnitude observed in this and past research. It did not prove that this area of tactics was the cause of all the variations observed, but did show that whatever the cause, tactical control of the details of maneuver coordination, *if feasible*, can control (remove) the variation. This work has thus finally proven a modified form of the hypothesis that Keith Myers of AMSAA stated when he was first acquainted with the initial [VRI, 1970] investigations of this topic. This hypothesis in its original form -- that military design of the tactics on different but reasonably similar terrains would eliminate any major variations in combat results on them -- was disproved in the 1973-74 research. We have now shown, however, that adaptive control of the details of fire and maneuver *can* control the

undue variabi ty of (predicted) combat results which has been observed in practice.

The fact that a player using such detailed control can beat the opponent in a simulation does not indicate

(1) that the information needed to formulate such detailed controls is available on a battlefield,

(2) that, even if the information required is available, the kinds of detailed coordination measures could be performed in actual combat, with its already extreme demands on commander and troops, or

(3) that there are not defensive counter-tactics or unsimulated penalties (such as might be involved in delays caused by coordination), which would cause real commanders to choose the risks of non-coordination over the dangers of coordination.

It is therefore not proven that such controls would be practicable or useful on the battlefield, and thus also not proven that such controls should be simulated in Army studies. However, the large potential utility which such control and coordination would seem to have on the basis of the present limited research seems to clearly justify further research on this topic.[1] Because of the complex human performance questions involved, this future research should clearly involve field experiments in which the performance demands on commanders and troops

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[1]Interested parties may wish to compare the results of this study with those of field experimental studies of the effects of platoon size on combat performance of units.

are as nearly real as possible. Additional work on tactical analyses,

attempting to identify the potential costs of such detailed coordination

and control as well as its benefits, would also be appropriate, as would

work to identify more clearly and precisely what types of exposure

correlation are most beneficial.

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PART I, CHAPTER 5: CONCLUSIONS

There are three major conclusions from the research results reported in chapter 4. The first is that quantitative measures of exposure patterns exist which strongly associate with predicted combat results in the engagement. The specific measures found are discussed in chapter 4, together with a discussion of their limitations. The measures do not themselves separate the effects of terrain from the effects of tactics.

The second conclusion was derived during investigations of the separate effects of terrain and tactics discussed in section 4.3. The conclusion is that variations in the degree of correlation of the exposure of attacker weapons is the most dominant identifiable effect in producing the variations of combat results with terrain and tactics. The research further indicates that control of the correlation of exposure by the attacker improves predicted combat results for the attacker significantly, as well as reducing the previously unexplained variations in combat results on terrains thought to be similar.

The third conclusion is that the potential value of improved control of exposure coordination in attacks justifies a program of analysis and field experimentation of the feasibility of such con-trol and on potential defender responses which could counter attacker initiatives. Field experimentation is necessary because of the serious questions of human information processing and decision making

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performance involved in the control of individual weapon exposure by
higher levels of commanders.

PART II

EVALUATING INTERPERSONAL DIFFERENCES IN THE

TACTICAL USE OF COVER AND EXPOSURE BY

INFANTRY AND ARMOR OFFICERS

PART II, CHAPTER 1: BACKGROUND

The Army makes predictions of the probable combat results of hypothetical engagements, and of the comparative results of different engagements, for many purposes. Most of these involve attempting to determine and compare the possible contributions which could be made to Army performance through alternative changes in weapon systems or force structure. In order to properly understand the information which his studies generate, the analyst must understand the factors which contribute to the predicted combat result and see to it that the measurements and comparisons he makes include only the data in which he is interested.

When one analyzes the predictions made in Army studies, one can breakout the contributing factors in the following conceptual equation:[1]

Predicted combat results

= predicted average combat results of a standard force on a standard terrain using standard tactics

+ differences due to variations in weapon systems characteristics in the force from those in the standard

+ differences due to variations in force structure from those of the standard

+ differences due to variations in terrain from those in the standard

+ differences due to variations in tactical decision making from that of the standard

+ differences due to variations in the interaction of terrain and tactics from those interactions in the standard case

+ differences due to variations in the interaction of weapon systems and/or force structures with terrain and/or tactics from those interpretations in the standard case

+ differences due to random variations in the details of the combat events (which rounds hit, etc.)

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[1]This equation is intended as strictly conceptual -- any real equation would almost certainly be non-linear in nature and would probably involve a slightly different set of terms.

With such an equation, one can analyze what happens when one takes the difference in predicted combat results for two situations -- say cases which differ only in weapon systems designs. The first term disappears as a common element which is subtracted from itself. So do the third, fourth, fifth and sixth. This leaves the following:

Predicted difference in combat results due to
a change in weapon systems design

   =   differences due to variations in weapon systems
       characteristics

   +   differences due to variations in the interaction
       of weapon systems, terrain, and tactics

   +   differences due to random variations in the details
       of the combat events.

The use of averages of several Monte-Carlo samples for each case can reduce the magnitude of the last term. Alternatively, deterministic combat models which attempt to predict average results, rather than a random sampling of results within their own computations, may be used to control this term. Historically in Army studies, the second term has then been neglected as un-important, and the comparison of results for the cases of interest has been taken to reflect differences actually due to the weapons changes of interest. Recent research work (see part I, chapter 1, and the reports referenced there) has shown that the second term can be significant, and in fact that combat model results and differences between cases may be more due to interactions between the terrain and tactics chosen for a study and the weapons and force structure studied than to the principal effect of real interest -- the systematic variation in effects which can be produced by a change in weapons or force structure.

This has suggested that some techniques or policies designed to control the magnitude of the second term may be required in Army studies. Such policies could involve the use of several terrain and/or tactical scenarios, rather than the typical single one or two samples that are presently used. In considering such policies, it is important to ask whether several individuals or teams should be involved in creating the tactical scenarios, or whether scenarios from one tactical planner or team could be expected to provide coverage of the real variability involved. Past studies of the relation between combat results and terrain and tactics have used individual scenario planners, and have therefore provided no information on this issue. This study was therefore undertaken to explore the kinds of systematic differences *in tactical use of terrain which could* be expected to occur in scenario design. While limited to map work and thus not specifically addressing the question of whether similar differences could be expected in real combat actions, it was thought that the results of the study might also suggest areas where field experimental investigation of possible real effects might be undertaken.

PART II, CHAPTER 2: RESEARCH PROBLEM

Previous research efforts investigating the relation between tactical exposure and predicted combat results in various scenarios have shown that significant variability can be expected between the predicted combat results achieved on different scenarios, even when the scenarios were designed on pieces of terrain chosen to be as militarily identical as possible and the scenarios were designed by a single individual. In view of the fairly surprising degree of variability shown in the original work, the project reported here was undertaken to explore the possibility that some kinds of variability in predicted combat results were associated with systematic differences in interpersonal tactical designs.[1]

The specific goals of this project, the first measurement of interpersonal differences in tactical behavior, were to determine whether systematic interpersonal differences in tactical decision making exist, and, if they exist, to identify the types of differences on each of five areas, specifically:

(1) task force organization,

(2) defender deployments,

(3) defender maneuver tactics,

(4) attacker route selection, and

(5) attacker maneuver coordination techniques.

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[1]An additional research effort (described in part I of this report) was also conducted to provide a better quantitative, analytic understanding of the originally observed variability, and to provide statistical measurements of exposure scenarios which can more closely predict combat model results on the scenarios. This statistical research, performed in the same period as these interpersonal variability experiments, did not address the interpersonal variation question.

In addition, because the experiments which would be performed would contain a significant base of data concerning tactical behavior unlike any which had been gathered in any earlier research, it was thought possible that some unplanned observations would be made that would improve analytic understanding of military tactical behavior.

Chapter 3 discusses the design of the experimental tasks, the choice of the experimental subject population, and the selection of the central quantitative analyses and contains as appendices copies of the experimental instructions which were used and reduced copies of the map materials which were used in the experiments. Chapter 4 describes the experimental results. Chapter 5 summarizes the major conclusions from the experiments.

PART II, CHAPTER 3: RESEARCH DESIGN

The design of the research program on interpersonal differences in tactical decisions governing the use of terrain and exposure will be discussed in this chapter in two sections: first, the design of the experiment which would produce data for analysis and, second, the design of the analysis of the resulting data.

3.1 Design of the Experiment

The first question which confronted the research team for this project was the selection of the appropriate subject pool for the experimentation. The research called for samples of 25 to 50 persons in two separate groups, to work on defensive and attacking tactics. (The sample sizes were identified as the minimum sizes in which interpersonal differences involving 25% of the subject population could reasonably be expected to be detected.) In order that this base would be as useful as possible for this study, it was hoped that it would be representative of Army officers who actually would make the tactical decisions involved in practice and also of those who would be used in scenario designs in studies. With the assistance of the study sponsor's office, the officer students completing the advanced officer courses at the US Army Armor School and the US Army Infantry School were identified as appropriate pools of subjects. With the cooperation of the schools, it was agreed that a group of approximately 30 to 36 officers at each school would participate in two day experiments toward the end of the course.

Given this source of officers as experimental participants, the question of how to mix the work on defensive tactics with that on attacking tactics arose. The two major alternatives considered involved (1) assigning the defensive experiment to one school and the attack experiment to another or (2) splitting each school's participants into two groups, with one assigned the defensive problem and one assigned the attack. The second grouping was appealing, since it appeared that both inter-branch and interpersonal differences would be analyzable. However, when preliminary statistical analyses were conducted of the degree to which statistically significant results could be expected in such an experiment, it was found that the reduction in the homogeneous sample sizes made plausible differences statistically unlikely to be detected. The first grouping, even though it did not allow for work on inter-branch differences, was therefore used. The Infantry School participants were selected for the defense experiment and the Armor School participants for the attack experiment.

In order to obtain information on systematic interpersonal differences and separate these from interactions between individuals and specific terrains, the experimental design had to provide for each participant to work on more than one terrain. With the limited amount of time which could be expected from each participant, the use of three terrains for each participant was chosen. In order to make the information obtained in this experiment as applicable as possible to current Army study processes and interests, it was decided that all the terrain samples should come from Europe.

Originally, it was hoped to use terrain areas for which detailed exposure overlays could be provided. Assuming such overlays were used by the experimental participants in generating their tactical information, the experiments could then involve the production of quantitative information on individual weapon system exposures. Such information, while not necessary to the general exploratory nature of the experiments, would provide potentially useful detailed information on some quantitative measures of the tactical control of exposure. Without the use of such overlays in the experiments themselves, data reduction from the experimental results could not be expected to provide useful information at the level of detail involved, since the experimental participants' decisions would be governed by their estimates of line-of-sight characteristics which would almost certainly differ in detail from the characteristics which might be measured in attempts at quantitative data reduction. This inconsistency between the line-of-sights assumed by the decision makers and those used in the analysis of their decisions would make any conclusions concerning the details of exposure behavior meaningless.

A review of the availability of overlay data indicated, however, that overlays including the effects of vegetation could not be available within the possible time period during which the experimental participants would be available. Accordingly, it was decided to use one area for which a large number of overlays excluding vegetation effects were available, and to provide these to the participants along with standard military maps of the area. The other two areas would have no overlay information available.

The specific overlays used were provided by the US Army Systems Analysis Agency (AMSAA). They were identical with overlays which had been used in the Anti-Armor Systems Program Review.

The availability of these overlays dictated the selection of the first terrain for the experiment. This terrain area was selected in the area near Hunfield, Germany, on map sheet L5324. A reduced-scale reproduction of this 1:50,000 map sheet is contained in appendix C to this chapter.[1] The other two terrain areas were selected as areas in which somewhat similar military situations could be presented (as to the length of the defensive FEBA, the size of the defensive force and attacking force, the general mission of the forces, etc.) so that inter-terrain comparisons would be meaningful. They were, however, selected to represent greater and lesser degrees of relief, visibility, and trafficability, so as not to unduly restrict the applicability of the study results. One area chosen was immediately west of the Hunfield area, an area surrounding Schlitz, Germany, on map sheet L5322. The third area was considerably north of the first two, and represented terrain with significantly less relief and with significant marshy areas. This terrain was in the Schneverdingen, Germany, area, and is represented on map sheet L2924. Reduced-scale reproductions of both these 1:50,000 map sheets are also contained in appendix C to this chapter.

[1]While higher-resolution map materials were desired for this study, the research team was unable to identify sources of 1:25,000 or 1:12,500 maps except for those of areas on which the experimental participants could reasonably be expected to have worked tactical problems in their course work. It was felt that the use of areas with which the students had already worked would suppress the interpersonal differences with which the study was concerned, and the use of 1:50,000 maps was therefore accepted.

The basic experimental design for both the attack and the defense experiments was similar. Each participant would be given map materials, and both verbal and written information describing a force structure, a brief, general description of the force mission, and instructions and materials for recording their solutions to the tactical problems presented (including both written responses on paper and graphical responses on acetate map overlays).

The Infantry School participants were expected to design a battalion task force defense of an approximately 15 kilometer front on each map sheet. The task force contained three mechanized rifle companies and one tank company. Their responses included information on possible enemy avenues of advance, defense team organization, defense team deployment, weapon system deployment, and movement. The Armor School participants were expected to design one to five battalion advances and attacks in the same general areas as the defense experiments,[1] with objectives to the rear of any enemy defenses. The battalion task forces contained three tank companies and one mechanized rifle company. They worked with each battalion individually, without being asked to provide information on any brigade-level structure or coordination measures. They were asked to identify team organization of the battalion task force, to identify avenues of advance and individual routes of movement of subordinate units, and to specify

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[1]With the number of battalion designs depending on the availability of time to design the attacks. Each battalion-level response was for a different avenue of advance.

the types of coordination and control measures that would be employed to control the separate units. Copies of the written instruction materials provided each experimental participant are contained in appendices A (for the Infantry School) and B (for the Armor School).

3.2 *Structure of the Analysis*

The research area pursued in this experimentation is so new that well-defined quantitative measures of the interesting variables do not yet exist. Thus the analysis design for the project began by taking the five general areas of interest (organization for combat, defensive deployment, defensive maneuver, attacker route selection, and attack coordination)[1] and formulating initial hypotheses to be tested in each of them. In addition, because of the lack of previous research on the topics addressed, a very general procedure of data analysis was designed to attempt to discover additional hypotheses or variables of interest and provide the possibility of making appropriate measurements, even though they had not been formulated in advance of the experiment.

In the organization for combat area, the initial hypothesis for test in the defensive experiment was: the organization will uniformly involve four teams (one per company in the task force). In addition to this

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[1]The sixth area, quantitative measures of weapon exposure, had been eliminated due to the problems discussed in section 3.1.

hypothesis for test, it was planned to measure the frequency distribution of the use of various company team structures and to examine (using the general methods discussed below) whether any systematic inter-terrain or interpersonal variations could be discovered. It was felt that the artificiality of the conditions of the attack experiment, in which five identical battalion task forces (each with four companies) were to be addressed, would limit the degree to which useful, realistic information on interpersonal variations in attack organizations would be obtained, and no measurements in this area were planned.

In the area of defense deployments, the single hypothesis selected initially for test was that Dragon deployments would be significantly more uniform than TOW deployments. It was planned to measure this in two ways: quantitatively, by taking the maximum distance between adjacent TOWs in a deployment and comparing it with 1.5 times the maximum distance between Dragons (the factor of 1.5 representing the difference in overall numbers of the two weapons), and qualitatively, by visual judgments. In addition to this hypothesis test, it was planned to measure the degree to which Dragon emplacements were forward or rearward of TOW positions. It was planned to examine, using the general techniques discussed below, the degree to which interpersonal differences in the exact deployments of platoon and company units and of TOWs could be discovered. These techniques were also planned to be used to examine the degree to which the estimates of defender unit positions in the attacker experiment agreed with the actual positions selected in the defense experiment.

In the area of defensive movement, it was planned to test the hypothesis that the distance to designated alternate or succeeding positions from their predecessors would be uniformly less than 500 meters. It was planned to use the general techniques discussed below to examine the amount and type of movement for interpersonal variations.

In the area of attacker route selection, it was planned to use the general techniques discussed below to identify interpersonal differences in the identification of routes of advance and, if possible, of specific attack routes against particular defensive positions. It was also planned to examine the degree to which the avenues of advance described by participants in the attack experiment agreed with the avenues of enemy advance described by the participants in the defense experiment.

In the area of attacker coordination measures, it was planned to test the hypothesis that coordination measures would be taken no more than once at each unit level (that is, company coordination would be specified at no more than one phase line for a battalion attack, and inter-platoon coordination at no more than one phase line per company attack). In addition, it was planned to examine coordination measures for interpersonal differences.

In each area where an examination of interpersonal variation in some item of interest was specified, the following general procedure was established for the examination:

    (1)   the data item involved would be examined for all the subjects on each terrain. Where the item involved graphical data, this would be done by examination of as many overlays as possible at one time (generally about 20). This

would give a picture of two things: the overall distribution of the data and the degree of agreement on various aspects.

(2) On this basis, one or more properties would be defined which appeared to divide the population involved. (Examples included the use of avenues of attack along roads.) These properties had to be formulated in some set of terms which could be translated to the other terrains (so that the use of a particular defensive position was not an acceptable property, although the use of the highest hill mass was).

(3) The results on all terrains would then be sorted on this property, and a statistical test would be performed to determine whether there were distinguishable systematic differences in individual subjects' responses.

(4) The specific statistical test applied was the χ^2 test of the hypothesis that the probabilities of a response satisfying the property involved were the same for each participant in the experiment. A rejection of this hypothesis at the five percent level was taken to indicate a confirmation of the existence of interpersonal differences. This test is described in most standard statistical texts (see, for example, chapter 9 of [Fleiss, 1973]). The statistic computed is

$$\chi^2 = \frac{1}{\bar{p}\bar{q}} \sum_{i=1}^{m} n_i (p_i - \bar{p})^2$$

where

m = the number of participants,

n_i = the number of terrains for which participant i gave data,

$$p_i = \text{the fraction of participant } i\text{'s responses}$$
$$\text{which satisfied the property under test,}$$
$$\bar{p} = \text{the fraction of all responses which satis-}$$
$$\text{fied the property } \bar{q} = 1 - \bar{p},$$

and the test is significant at the 5% level if $X^2 > \chi^2_{m-1,.05}$,
the 95-percentile point of the χ^2 distribution with m-1 degrees
of freedom.

As the first experiment ever conducted on its topic, this study was
not able to draw on a set of tested definitions of data items and quantitative
measures which were known to measure the subjects of interest. As made
clear above, although some quantitative data items were formulated in ad-
vance, much of the study process was designed to first identify interesting
hypotheses to be further examined by qualitative and subjective review of
the experimental results, and then to use statistical techniques to measure
them. This method makes the usual performance properties of standard
statistical tests, including the χ^2 test used in this research, meaningless,
because statistical performance measures are conditional on the test having
been formulated without review of the data.

To illustrate this problem, consider a case in which an experimenter
desires to determine whether a particular probability is greater than 50%.
Standard tests for the value of a proportion are available such that they
will conclude that the hypothesis is true when in fact it is false no more
than 5% of the time. Now suppose that there were, in fact, 30 different
probabilities which one might have chosen to test and that one first
chooses to examine the data to identify which probability

among them seemed most likely to be greater than 50%, and then applied
the standard test to this probability only. This composite process will
have a probability of concluding the hypothesis is true when, in fact, it
is false of up to 79%, rather than the 5% error rate which the formal
test was designed to produce. In our research, where there are an almost
infinite number of measurements which could be made on the data and only
those which the data suggests will be made, this problem is significant.
In every case of this kind where data was reviewed before the measurements
applied to it were defined, this report will make clear that this occurred.
In such cases, the statistical levels of significance reported for any data
analysis must be considered only as descriptions of the degree of conformity
between the data and the hypothesis involved, rather than as formal limits
on error rates.

Chapter 4 describes the results of these experiments, and chapter 5
summarizes the major conclusions reached from them.

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APPENDIX A

TO PART II, CHAPTER 3

INFANTRY SCHOOL EXPERIMENT MATERIALS

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BACKGROUND

Major questions have recently been raised concerning the methods currently used to represent terrain effects in war gaming and simulation analyses of armored and mechanized company and battalion combat. We are presently studying some of these questions for the Office of the Deputy Undersecretary of the Army. Among the questions we are addressing is the degree to which individual decisions on weapon deployments differ, and the potential effects of such differences on the results of Army studies which use only one deployment. To assist in answering this question, we have asked TRADOC to allow us to run an experiment in which you will determine weapon deployments which we will use in standard war gaming and simulation methods. You will be asked to do this in each of three situations. In all cases, the forces involved are those shown on the figure on the next page.
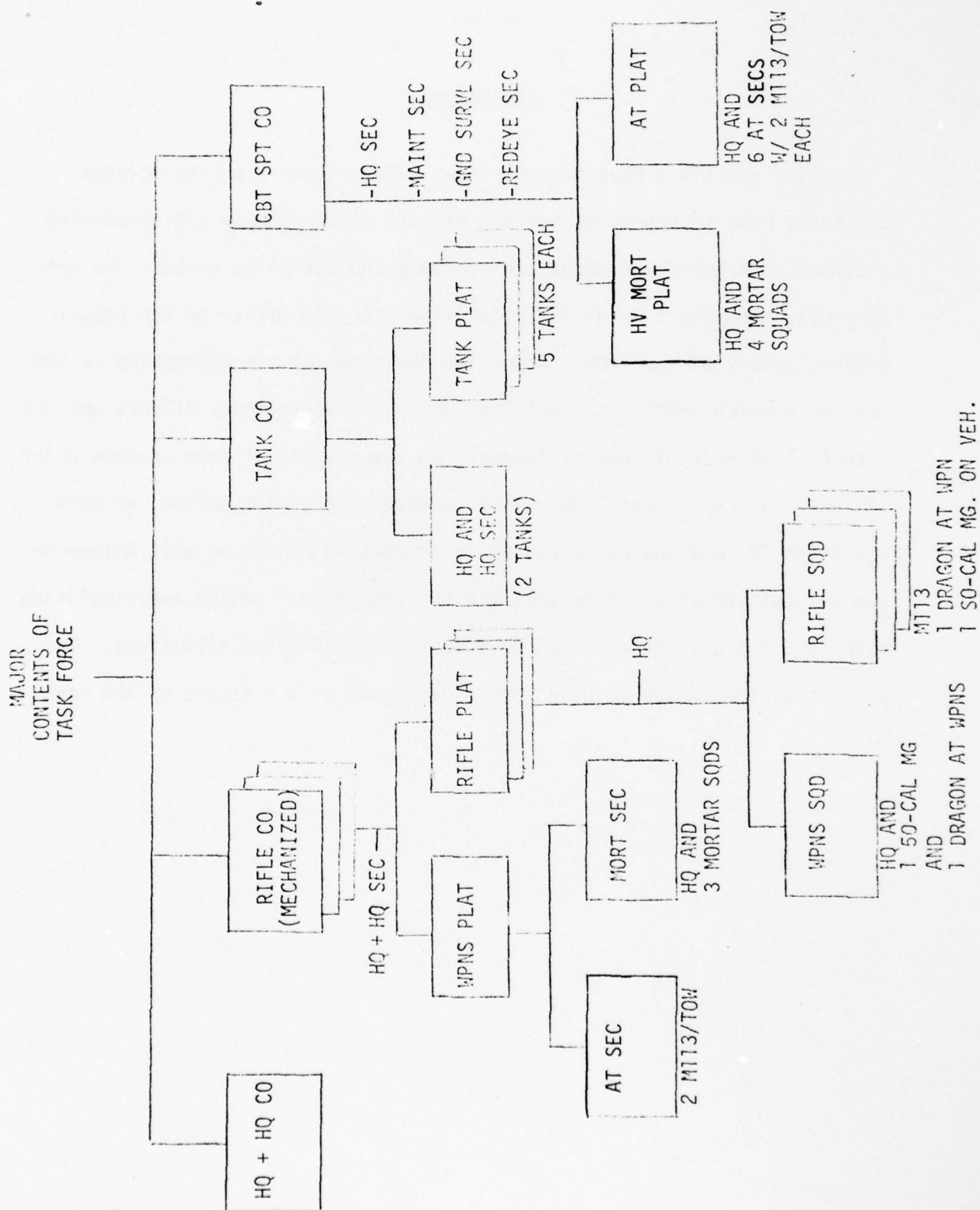
MAJOR
CONTENTS OF
TASK FORCE

HQ + HQ CO

RIFLE CO
(MECHANIZED)

HQ + HQ SEC

WPNS PLAT

AT SEC

2 M113/TOW

MORT SEC

HQ AND
3 MORTAR SQDS

RIFLE PLAT

HQ

WPNS SQD

HQ AND
1 50-CAL MG
AND
1 DRAGON AT WPNS

RIFLE SQD

M113
1 DRAGON AT WPN
1 50-CAL MG. ON VEH.

TANK CO

HQ AND
HQ SEC

(2 TANKS)

TANK PLAT

5 TANKS EACH

CBT SPT CO

-HQ SEC
-MAINT SEC
-GND SURVL SEC
-REDEYE SEC

AT PLAT

HQ AND
6 AT SECS
W/ 2 M113/TOW
EACH

HV MORT
PLAT

HQ AND
4 MORTAR
SQUADS

FIGURE 13

ASSUMED SITUATION

SITUATION: Three situations will be presented, in each of which a
high speed armor and mechanized infantry attack is expected
across a wide front with the intention of breaking through
defending US forces. In none of the three cases in this
experiment is it expected that the major enemy attack will
be in the brigade area involved, although serious secondary
attacks are expected, with force ratios of approximately
three or four to one probable. In all cases, the season is
summer, the weather and the trafficability are good, and
although a daytime attack is expected, the defense forces
must also be prepared for night operations.

MISSION: In all three experimental cases, the general mission of the
forces to be deployed is the area defense of a sector of the
overall defensive line. Although success must be anticipated,
the positions taken must permit an orderly withdrawal under
pressure in case an enemy breakthrough elsewhere necessitates
a defensive realignment. The specific defensive areas are
designated on the pages with the map sheets involved. Corps
and division security forces are expected to give between 20-
minutes and 1-hour warning of actual enemy attack, but are not
expected to significantly damage or delay the enemy forces.

ASSUMED SITUATION

(Continued)

SUPPORT: Because the main attack--expected in a different area--is

anticipated to involve force ratios of five or six to one,

each deployment in the three experimental situations should

depend only on the organic and attached forces of the task

force involved and on artillery support for final protective

fires only. No other air, artillery, or other support, should

be expected. Normal resupply may be anticipated. No

minefields or other barriers, or demolitions may be assumed

to have been constructed or prepared for any of the areas.

GENERAL INSTRUCTIONS

For each situation, *you will be* asked to:

Designate an organization for combat.

Designate areas for company- and platoon-level force elements.

Designate the most threatening avenues of attack.

Designate areas for final protective fires.

Locate headquarters, forward observers, and major weapon systems,

including:

battalion and company headquarters

forward observers

M113/TOWs

tanks

dragon AT weapons

mortars

M113s, and

machine guns,

including any movements or alternate positions.

Specific instructions on formats for these are on the next page.

SPECIFIC INSTRUCTIONS

1. ORGANIZATION FOR COMBAT. Please sketch the organization of company teams and/or other force elements which you would anticipate using in such a defense on the Organization for Combat response sheet provided. Please place your identifying number and the letter code for the situation in the upper right hand corner of the sheet.

2. ORGANIZATIONAL AREAS. Please identify on an overlay in BLUE the areas to be occupied by each platoon-level and above force element. Any form of sketching and labeling is acceptable. In order to permit correlation of the overlay with the map, please place BLACK X's on three coordinate intersections and identify their coordinates on the overlay. Place the printed label identifying you, this situation, and overlay #1 on the overlay. Do not put the overlay aside; you will be using it further.

3. AVENUES OF ADVANCE. Please identify in RED on the same overlay used for the organizational areas data the major enemy avenues of advance which you are defending against. Do not put the overlay aside; you will be using it further.

4. FINAL PROTECTIVE FIRES. Please identify in GREEN on the same overlay the areas for artillery and mortar fires in the final protective fires. Put this overlay aside, being careful not to smear the ink used to record the data on it.

5. COMMANDERS AND FORWARD OBSERVERS. On a new overlay, please identify in BLUE the locations of the battalion and company commanders and in GREEN the location of forward observers. Identify in GREEN also the general areas in which the greatest degree of each forward observer's attention would be directed. Please identify on the overlay in BLACK three coordinate intersections and place your overlay 2 label for this situation on this overlay.

6. M113/TOW LOCATIONS. On a new overlay, identify in BLUE the positions which your M113/TOW vehicles would occupy, with alternate locations in GREEN and displaced positions, if movement will occur, in RED. Where deployments will involve several weapons within 50 meters of one another, a single point or X may be used to identify the location, which should be labeled with the number of systems involved. Please identify three coordinate locations in BLACK and place your overlay 3 label for this situation on this overlay.

7. TANK LOCATIONS. On a new overlay, identify tank locations in the same manner used for TOW locations previously. Identify three coordinate locations and place your overlay 4 label for this situation on this overlay.

8. DRAGON LOCATIONS. On a new overlay, identify dragon locations in the same manner used for TOW locations previously. Identify three coordinate locations and place your overlay 5 label for this situation on this overlay.

9. MORTAR LOCATIONS. On a new overlay, identify mortar locations in the same manner used for TOW locations previously. Identify three coordinate locations and place your overlay 6 label for this situation on this overlay.

10. M113 LOCATIONS. On a new overlay, identify M113 locations in the same manner used for TOW locations previously. Identify three coordinate locations and place your overlay 7 label for this situation on this overlay.

11. MACHINE GUN LOCATIONS. On a new overlay, identify heavy machine guns locations in the same manner used for TOW locations previously. Identify three coordinate locations and place your overlay 8 label for this situation on this overlay.

SITUATION C

This situation is on the HUNFELD map sheet (L5324). The battalion defense is to be conducted along the general line from Arzell to Hofaschenbach (NB 555240 to NB 600100).

In this situation, some additional materials are available which might possibly assist you in your analysis of the situation. These are a set of transparent overlays showing the line-of-sight properties of various locations on the map. Each separate overlay shows the area which is intervisible with a particular point on the map--DISREGARDING ALL VEGETATION EFFECTS AND ACCOUNTING ONLY FOR ACTUAL TERRAIN INTERVENTION. The points for with which such overlays are available include both road points and off-road points, and might be either enemy or friendly positions at some point in any combat action. The points for which overlays are available are listed on the following page. On each overlay, the clear area is visible (disregarding vegetation) to the observer or weapon at the location of the triangle. Other areas labeled "1" will allow visibility of the top of vehicles, but not of the entire hull. Other areas are totally masked. These overlays were designed for another use entirely, and may prove useless or too difficult to use for this experiment. Their presence in this material should not be taken as an indication of potential locations or a suggestion that they will be fact prove useful.

SITUATION C

POSITIONS FOR WHICH OVERLAYS ARE AVAILABLE
(listed in order that overlays are stacked)

```
5869 1732
6272 1194
5777 2320
6052 2500
5962 2180
5613 1653
5632 1567
5772 1732
5771 1572
5986 1081
6169 1084
6091 1150
6160 1180
6156 1093
5446 2412
5627 2425
5745 2389
5628 2302
5502 2456
5715 2556
5727 2507
5826 2377
5889 2452
5967 2426
5778 2239
5821 2368
5783 2252
5873 2182
5687 1640
6116 1131
5669 2356
5824 2480
5843 2240
6253 1574
5697 1388
5372 1935
5692 1976
5664 1945
5897 2062
```

SITUATION N

This situation is on the SCHNEVERDINGEN map sheet. The battalion defense
is to be conducted along the general line of the major north-south road
from the intersection at approximately ND 568897 to the intersection at
approximately ND 572758.

SITUATION X

This situation is on the LAUTERBACH map sheet, although you may wish to use the neighboring HUNFELD map sheet for reference also. The defense is to be conducted along the general line of the Fulda river from a position roughly between Unter Wegfurth and Ober Wegfurth on the North to a position roughly between Ullershausen and Hartershausen on the South.

APPENDIX B

TO PART II, CHAPTER 3

ARMOR SCHOOL EXPERIMENT MATERIAL

BACKGROUND AND INTRODUCTION

Major questions have recently been raised concerning the methods currently used to represent terrain effects in war gaming and simulation analyses of armored and mechanized company and battalion combat. We are presently studying some of these questions for the Office of the Deputy Undersecretary of the Army. Among the problems we are addressing is the degree to which individual decisions on tactics for utilizing terrain for weapon deployment and movement differ, the nature of the common elements in such decisions, and potential ways to improve the representation of these tactics in the Army study process. To assist in answering this question, we are conducting, with TRADOC's assistance, experiments at the armor and infantry schools in which you and other qualified officers will determine weapon maneuvers and deployments which we will use in standard war gaming and simulation methods. In doing this, you will be asked to address questions which would normally be analyzed at several levels of command, from brigade to platoon, with the greatest attention spent at the battalion and company levels.

You will be asked to do this for several battalions in each of three general situations--designated cases D, L, and X. Each of these deals with a different map area. Case D deals with areas on the HUNFELD map sheet, case L with areas on the SCHNEVERDINGEN sheet, and case X with the LAUTERBACH sheet. You should have one of these map sheets at your location. You should also have a set of preprinted labels with a unique number identifying you, a case code, and an overlay number. You will use these to attach to the overlays you produce on this experiment. The

case code of the first column of labels should correspond to the map at
your position. In each of the three cases you will be performing portions
of a terrain analysis to support an East-to-West attack in certain portions
of the map, and then outlining the details of one or more battalion attacks
on the basis of this analysis.

If your number (see preprinted labels) is 1,4,7,10,13,16,19,22,25,
28, or 31, you will work with the cases in the order D, L, X. If your number
is 2,5,8,11,14,17,20,23,26,29, or 32, you will work with them in the
order L, X, D. If your number is 3,6,9,12,15,18,21,24,27,30, or 33, you
will work with them in the order X, D, L.

Case D is concerned with an area on the HUNFELD map sheet. You should
assume that friendly forces of interest begin in various locations near
the Ulster river from coordinate 120 on the south to 280 on the north and
attack generally to the west with a slight southerly direction, leaving
the map sheet at locations from coordinate 060 on the south to 220 on the
north. You should assume that no defensive forces will be encountered
east of the 640 coordinate.

Case L is concerned with an area on the SCHNEVERDINGEN map sheet.
You should assume that friendly forces of interest begin in various loca-
tions near or off the eastern edge of the map from coordinate 750 on the
south to 900 on the north. The attack is directly to the west. You
should assume that there will be no enemy forces east of coordinate 630.

Case X is concerned with an area on the LAUTERBACH map sheet. You
should assume that friendly forces of interest begin off the eastern edge
of the map from coordinate 100 on the south to coordinate 250 on the north
and that they are to attack directly to the west. You may assume that
there are no major enemy forces east of the Fulda river.

In all cases, you should assume that each battalion with which you become concerned is a battalion task force consisting of an armor battalion augmented with a mechanized rifle company, including two M113/TOW vehicles. All of the attacks are to be conducted with the mission of breaking through the defensive positions and penetrating to a depth of 30 to 50 kilometers within 2 days. This experiment is not concerned with the entire process, but principally with the encounter with the main defensive position. Accordingly, you should assume in each case that your forces begin in convenient locations just east of the area of interest for the situation as outlined above, and that their objectives are west of the area of interest. Your problem in this experiment is concerned with attacking, defeating, and breaking through or disorganizing the defending forces. The ensuing pursuit, exploitation, and/or continued drive to the objectives are not to be addressed. It should be assumed that all successes will be followed up by major reserve elements which will both enlarge the breakthrough and perform extensive exploitation.

In all cases you should disregard all present political boundaries. You may assume, in spite of the direction of attack, that the enemy forces are Soviet and Warsaw Pact forces. For each battalion it should be assumed that a main attack is being made elsewhere, and that no air, attack helicopter, or other support beyond limited artillery support should be anticipated. Wherever relevant, all useful engineer support for bridging rivers should be assumed. In all cases, the season is summer, the weather and trafficability are good, and the attack is a daylight attack.

The overlays and other information to be produced for each case are similar, and a single set of general instructions is given on the following pages.

OVERLAYS 1 AND 2--PRELIMINARY TERRAIN ANALYSES

Please identify at least five avenues of East-to-West movement suitable for the use of battalion-level forces in the general mission and area described earlier. Sketch these on an acetate sheet, grouping them until they finally show areas for exactly five battalions; mark three coordinate positions so that the acetate may be correlated with the map again; and place your preprinted identification lable for OVERLAY 1 for this situation on the overlay.

On a second acetate, please identify in different colors, at least one and up to three alternative defensive deployments which might be employed to defend against such an attack. If you cannot find three reasonable defensive deployments which differ throughout, these may be identical in some areas and different in others. If you can identify more than three alternatives, please use the strongest ones which differ significantly. If you cannot identify three alternatives within the terrain area of interest, please outline at least one. For each defense, sketch the general line of the defense, identify major strongpoints within it, and show the particular points which you would expect to be the greatest threat to forces attacking along the avenues you identified in overlay 1. Label this overlay with your appropriate OVERLAY 2 label and make sure that three coordinate positions have been marked so that it can be correlated with the map again.

OVERLAYS 3, 4, 5 AND 6--PLANNED ATTACKS

From your work with avenues of attack (OVERLAY 1) and defensive positions (OVERLAY 2), you should now have five battalion attack situations against each of three posible separate defensive lines. The next three overlays are concerned with these situations, one overlay for each defensive line. For each overlay, please design an attack for at least one of the five battalions, considering for each attack the problems of the battalion commander and staff officers involved. Attacks should be designed for as many battalions as time permits. Assume that good intelligence reports have located the enemy in the general positions involved. Your overlay should only deal with the last 3 to 6 kilometers in front of the defense. In addition to the battalion-level problems, please also consider the problems of each company commander, so that in the complete design for an attack, the routes for each platoon are given, with all battalion- and company-level phase lines and/or other coordination points marked. Please describe briefly on the paper answer sheets provided the organization of the battalion for combat and the specific scheme of attack for the battalion and the companies, including all coordination measures to be used and any planned conditions for the use of the reserve forces (if any reserve is to be used.) These prose descriptions need not repeat material which is similar for the various battalions against a single defensive line, or even against several defenses, but may simply note any significant differences in the meaning of the phase lines, the methods of coordination, etc. In addition to this information, please describe the formation or deployments of the force you would expect to be used both before the attack and as the attack is conducted. This information is needed only for any periods where the

1.0

1.1

1.25   1.4   1.6

2.8   2.5
3.2   2.2
3.6
4.0   2.0

1.8

MICROCOPY RESOLUTION TEST CHART
NATIONAL BUREAU OF STANDARDS 1963 A
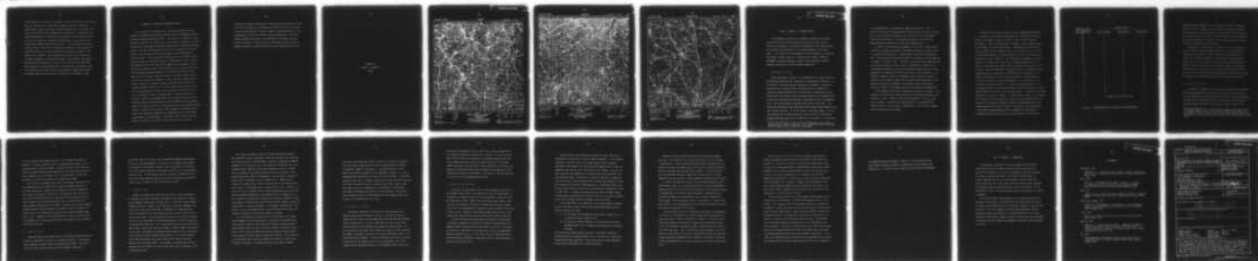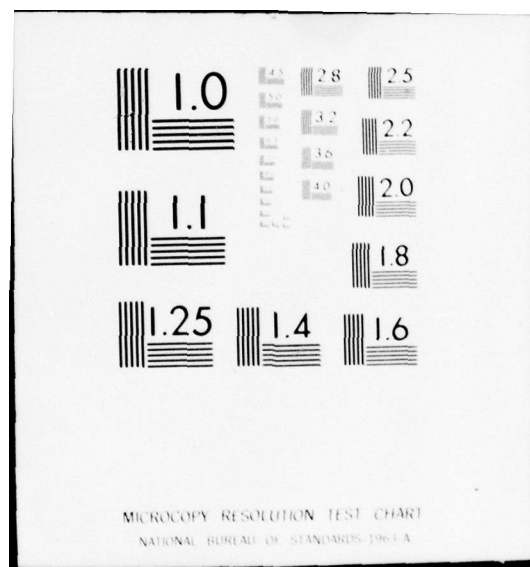
overlay sketch is not clear or for levels of detail below that of the overlay. Thus, for instance, if at some point companies would be following one another down a single road with a kilometer between them, the overlay would show a single route, and the formation should be noted. Additionally, formation information for vehicles within individual platoons, which is below the level of the overlay, should be given wherever it would differ from a standard (the standard should also be given). This information should be given no matter at what level of command the information would actually be determined--that is, you are here considering battalion-, company-, and platoon-level problems. Again, please be brief and omit all repetitions of common information for the various attacks. When complete, you should have three overlays labeled 3, 4, and 5, each with one to five battalion attacks against a single defensive line, each with three coordinates marked for correlation to the map, and one or more pages of answers which should be stapled together and to which you should attach your OVERLAY 6 label.

OVERLAYS 7,8,9 AND 10--UNPLANNED ATTACKS

In your work on the overlays 3,4,5, and 6, you assumed that each attack was planned on the basis of good intelligence information locating the general enemy defensive positions. For overlays 7 through 10, please assume instead that the overall plan of the battalion involved in each case was based on intelligence information indicating that there were no enemy forces in the area involved, and that the first information to the contrary is when the enemy force involved opens fire on the advancing friendly forces. Please assume that the enemy lets the friendly forces advance so that a significant fraction of the advancing force is within the 3000-meter effective range of the SAGGER anti-tank weapons, even if this means allowing the most forward elements to just within 1000 meters. (If the planned movement formation for the battalion would not bring major parts of the force within the range of the enemy weapons without bringing the most forward elements in closer than this, assume that the forward elements are allowed to within under 2000 meters and are destroyed by intense, effective fire, with no report of the source of the fire to the main body of forces--only of the fact that they are under fire, followed by a break in communications.) Assuming that such an encounter leads to an immediate hasty attack, please describe in these overlays (using the answer sheets provided as OVERLAY 10) how you would expect the attack to evolve, in exactly the way you described the planned attacks in overlays 3,4,5, and 6. Please remember in constructing your answers the limitations imposed by the immediate nature of the attack on the degree of command/control coordination which could be expected, and be as realistic as possible. Please be sure that your prose

description includes a description of the pre-encounter formation of the
forces and distinguishes the command and control decisions and actions
of the various commanders from one another, so that battalion-level and
company-level reactions and actions can be distinguished. As in the
planned attacks, please omit all repetitions of information for the
various situations. Please describe *briefly* what you might expect to
happen differently if radio communications were impossible due to ECM.

APPENDIX C

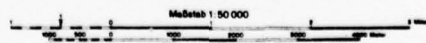TO PART II, CHAPTER 3

MAPS

Deutschland 1:50000   Hünfeld   Ausgabe 4-DMG   Serie M 745   L

Maßstab 1:50000

Universale Transversale Mercatorprojektion

Serie M 745
Ausgabe 4-DMG   L 5324   Hünfeld   STOCK NO M745XL5324

Deutschland 1:50 000

Lauterbach

Ausgabe 1-DMG   Serie M 745   L 532

Maßstab 1:50 000

Universale Transversale Mercatorprojektion

Serie M 745
Ausgabe 1-DMG   L 5322   Lauterbach

Deutschland 1:50 000   Schneverdingen   Ausgabe 1-DMG   Serie M 745   L 2924

Staats

forst

Langeloh

Sellhorn

Schneverdingen

Staat

Kloster   Bispingen

Langeloh

forst

Soltau

Walsrode

Wintermoor

Insel

Grauen

Lünzen

Schwalingen

Sprengel

Hützel

Brockhof

Delmsen

Neuenkirchen

Wilmerdingen

Wintermoor

Walterdingen

Lünzen

Wesseloh

Maßstab 1:50 000

Universale Transversale Mercatorprojektion

Berichtigungsstand

| B 1980 | D 1980 |
| S 1980 | N 1980 |

B = Berichtigung   N = Nachträge

PART II, CHAPTER 4:   RESEARCH RESULTS

The experiments to explore for systematic variability in the tactical use of terrain were conducted in June and July 1976, with 29 officers participating in the defensive experiment at the Infantry School and 29 officers participating in the attack experiment at the Armor School. The results of the experiments will be discussed in six sections, describing in order results on: organization for combat, defensive deployments, defensive maneuver, attacker route selections, attacker maneuver coordination, and miscellaneous observations.

4.1 *Organization for Combat*

As was described in chapter 3, the organization for combat results of the defensive experiment was examined for interpersonal variability. As can be seen in the data package in appendix A to chapter 3, the participants were given a battalion task force which had four companies. A hypothesis had been identified for testing that the organizations given would contain four maneuver teams (excluding mortar and air defense units). Although four-team cases were dominant, the hypothesis was not completely true: 34% of the responses contained either three or five teams. The three team organizations were organized around the mechanized companies, with tank platoons attached: no role was given to the armor company. The five-team organizations involved organizing a major portion of the anti-tank platoon either with the scouts or under task force control.[1] Of the total

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[1] The use of the scouts as a unit without reinforcement was not coded as a separate team in the data reduction. Thus, there were actually some additional five-team cases, if one counts the scouts.

of 86 organizations, 5 involved three teams and 24 had five. (It is possible that some of the organizations coded as having five teams actually had six, with the scouts assigned a unit role and the anti-tank platoon under task force control: the coding method used to classify the responses would have coded this as a five-team organization.)

In spite of the diversity of responses on this item, no statistically significant systematic variation in the number of teams with either terrain or individual could be found. A methodological remark may be appropriate here: the statistical test being applied in this analysis, as was described in chapter 3, was the χ^2 test. In practice, in order to limit data reduction and calculation effort, the test statistic (denoted X^2 in chapter 3) was calculated incrementally, with calculation being stopped as soon as it was mathematically impossible for the final statistic to be significant. This technique, adopted to control the amount of effort going into the data analysis in this research, means that the exact significance levels of the complete test were not computed in cases where the fact that the test would fail at the 5% level became clear part way through the computations.

This procedure should not be confused with sequential statistics. The statistical test which was being performed was the previously-defined, non-sequential χ^2-test. However, since only the question "Is $X^2 > \chi^2_{m-1,.05}$?" was of interest, if partial data indicated that it was mathematically impossible for X^2 to pass the test, computation on that item was stopped, and the negative result recorded.

In addition to the number of teams, the team contents were examined

for variability. Each separate subordinate unit identified in the exami-

nation of the number of teams was coded in terms of three variables:

the number of rifle platoons present, the number of tank platoons present,

and the number of attached TOW sections. Headquarters and related units

were not coded, nor were attached scouts or air defense units.) In terms

of these three variables, the participants used a total of 34 separate or-

ganizations in their responses. The distribution of responses is shown in

figure 14. No statistically significant association between the use of a

specific organization and either terrain or individual participants could

be found. Thus, although there is extreme diversity in the maneuver teams

formed by officers in the same or different tactical situations, no sys-

tematic interpersonal (or inter-terrain) variations could be detected.

The kind of variability in tactics detected here -- variability in the

structure of the force involved -- is normally examined explicitly in an

Army weapon system or force structure study, and does not appear to provide

any new source of variability in such study results. It may affect the inter-

pretation of such results, however. In most current studies, it is assumed

that changes in force structure at one level will be reflected fairly sys-

tematically in consequent changes throughout the subordinate units, the

combat performance of some one of which is examined. Thus, the addition

of new TOWs to mechanized battalions is assumed to be reflected in a

reasonably predictable and uniform way in the contents of the "typical"

Number of Times This Organization Was Called For	ORGANIZATION		
	Rifle Platoons	Tank Platoons	TOW Sections
53	2	1	1
44	3	1	2
37	3	1	1
30	2	1	2
27	1	1	1
26	3	0	2
24	3	0	1
21	1	1	0
14	2	0	1
12	2	1	0
9	3	0	0
8	2	0	2
6	2	0	0
4	1	1	2
4	3	1	0
3	0	1	0
3	1	0	1
3	3	1	3
2	4	1	2
2	4	0	1
2	2	1	3
2	1	2	2
1	(12 other cases used once each)		

FIGURE 14: DISTRIBUTION OF USE OF VARIOUS TEAM ORGANIZATIONS

company team whose combat results are examined. Because of the large variability that it appears should be expected in the actual company teams that would be formed and because of the significant non-linearity of the combat results with force structure, some explicit consideration of this variability and its affects on the average performance improvement may be appropriate to Army studies.[1]

An approach to controlling any biases due to this effect would be to sue the force-structure dependent data which is already typically produced in Army studies to produce reports of the distributions of results (or of weighted averages of results) which could be expected under a mix of force structures, and to examine how this would vary as weapons or larger scale force structures were changed. This approach would generally involve little if any additional data generation, but would correct the interpretation now being made of the results of studies to properly reflect the expected diversity of combat organizations.

4.2 Defensive Deployments

As explained in chapter 3, it had been planned to test the hypothesis that Dragon deployments would be significantly more uniform in density than TOW deployments by both quantitative and visual inspection techniques. The quantitative techniques had all been constructed on the assumption that the deployment responses would generally contain locations for

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[1]The present techniques for studies seem to be based on the combat results of the mean company, which will differ from the mean combat results of the companies if there is large variability in their makeup and the combat performance is a non-linear function of makeup, both of which appear to be true.

18 TOWs and 27 Dragons, the numbers in the task force structure given the participants. In fact, the number of TOWs located on the participants' responses ran from 12 to 20, and the numbers of Dragons from 21 to 27. This variability, for which no cause could be found, prevented the use of the quantitative measure designed for this test.

It was felt that this problem would not prevent the visual test, and it was performed. Each participant's responses were presented to an investigator, and a judgment was made as to which was the more uniform. Of the 79 sets of responses judged (some of the responses were unusable for various reasons), the Dragon deployments were judged to be more uniform in 70. This result must be examined with caution, as no tests of judgment accuracy against quantitative measures could be made.

In addition to this examination of the uniformity of deployment densities, measurements were made of the average distance which the Dragon deployments were forward or rearward of the TOW deployments. Neglecting three outliers in which TOW deployments were generally more than three kilometers forward of the remainder of the defense (and were to displace back during the action), Dragon deployments were found to be forward of the TOWs by 250 meters with a standard error in this mean difference of 45 meters.

TOW deployments were also examined for interpersonal variability. Classifying TOW locations from different responses as identical if they

appeared to be within 200 meters of one another,[1] 74 distinct TOW

locations were counted on the one terrain, 70 on another, and 62 on the

third. This indicated a significant variability in the location of the

TOWs. However, nearly half the total locations in each instance were in

the rear area (more than two kilometers behind the forward TOW deployments).

Thus, there was much less diversity in the forward deployments of TOW.

Grouping the responses into randomly selected pairs on each terrain, an

estimate was made of the fraction of identical forward TOW positions in

each pair.[2] The mean fraction of identical forward TOW positions was 52%,

with a standard error or estimate of 4%.[3] No statistically significant

systematic interpersonal or inter-terrain differences could be found.

(Three TOW deployment properties were examined for statistical significance,

but found to have none: these were the use of pairs of TOWs versus indi-

vidual TOWs, the initial use of TOWs forward of the general defensive line

as opposed to initial placement in the main defense, and the use of groups

of four or more TOWs in less than one kilometer of front. None of these

properties was found to have any systematic interpersonal variation.

Similar pair-comparison measurements were made of the location given

for companies or platoons. In these measurements, the decisions as to

whether two locations were identical were made on the basis of whether

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[1]Which was felt to be reasonable for the map materials and data collection
techniques employed.

[2]The fraction of identical positions was taken to be the number of positions
taken in both deployments divided by the number of forward TOWs in the two
deployments. As was mentioned above, positions within 200 meters of one
another were judged identical.

[3]These fractions account for only the forward TOWs. As was noted above,
TOWs in the rear were significantly less correlated between samples.

or not the symbols used shared some area. This allowed the centers of platoons in the same location to be up to about 600 meters apart in the responses drawn by the participants. The mean fraction of unit locations identical between randomly paired responses (on the same terrain) was 41%, with a standard error of estimate of 5%. No statistically significant systematic variations with terrain or individual were found.

The locations of defender units given in the defense experiment were also compared with those identified for potential defense locations in the attack experiment. Because of the freedom of the attack experiment participants to estimate the location of the defensive line, and thus to place their defensive positions in areas which did not agree at all with those assigned to the defense experiment, only a limited sample of 20 attack-defense comparisons could be made. In these, the pairwise agreement between attacker location estimates and defender location estimates was statistically indistinguishable from the overall defender-defender agreement. Thus, it appears that all participants will agree on the locations for defending units (given a specification of the general line of defense) about 40% of the time.

4.3 *Defensive Maneuver*

Defensive maneuver was specified in the responses by graphical presentations of movements to alternate or succeeding positions. The displacements suggested for TOWs, tanks, and Dragons were examined. While more than 75% of such moves were less than one kilometer, there were a

significant minority of cases in which large displacements were designed.
The use of large displacements (in excess of one kilometer) was examined
for systematic interpersonal variation, and no statistically significant
association between individual participants and the use of large movements
could be found. (A side analysis was also conducted to explore the possi-
bility that large movements were statistically associated with particular
terrain, but no evidence of such association was found.)

4.4 Avenues of Advance

Avenues of advance were identified for attacks in both experiments.
In addition, the attack experiment included the identification of specific
attack maneuver routes down to platoon level for the final attacks against
defender locations. Because of the diversity of individual defender loca-
tions used in the attack experiment, no population of comparable cases
could be found which was large enough to allow statistical analysis of the
final attack routes. A purely qualitative review of the attack route data
did turn up one potentially interesting observation, for which no quantita-
tive measure was identified. In a significant minority of the armor experi-
ment attacks, attacks were designed to bypass defender locations and come in
at the flanks, even where such maneuvers would expose the units involved to
fires from two different defender concentrations. This kind of maneuver
had not been observed by the project team in the scenarios they were
familiar with from Army studies. This suggests a possible need for some
examination of the effects on study results which could be expected if such
scenarios were used.

The avenues of advance identified by the experiment participants were examined using the random pair comparison techniques described above with the discussion of TOW deployments. Routes of advance were judged to be identical if they were similar in position and orientation for a four kilometer length immediately outside the defensive positions. Random pairs of responses from Infantry School officers participating in the defense experiment showed a 55% agreement in the location of avenues of advance (with a standard error of estimate of 4%). Random pairs of responses from Armor School officers participating in the attack experiment showed a 51% agreement (with a standard error of 3%). However, random pairs with one response from the defense experiment and one from the attack experiment showed only a 26% agreement (with a standard error of 3%). Attempts to explain this difference by a possible tendency of armor participants to identify more road-following avenues showed no statistical significance to such an explanation. No systematic interpersonal, inter-terrain, or inter-experiment effects could be identified to explain the apparent difference in the selected avenues of advance.

In considering the possible causes of the difference, the reader should remember that the participants in the defensive experiment were being asked to identify likely Warsaw Pact avenues of advance against their defense, while (in spite of the East-to-West orientation) participants in the attack experiment were asked to identify possible United States avenues of attack against a Warsaw Pact defense. This difference in the assigned roles of the participants may account for some of the differences observed. The reader should also note that the attack

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experiment participants were asked to identify five avenues of advance,
while the defense experiment participants were asked only to identify
an indefinite number of such avenues. The actual responses in the
defense case typically showed 3, 4, or sometimes 5 avenues. It is also
noteworthy that approximately one-third of the participants in the attack
experiment made verbal statements indicating that they did not believe that
five battalions would be given as narrow a total attack front as they
were asked to deal with (approximately 24 kilometers). While these
experimental differences may account for the differences, there may be
other systematic causes for the differences in the responses which were
not able to be identified in this study.

4.5 *Coordination of Maneuver*

The apparent importance of the details of the coordination of
maneuver among attacking elements (see part I, section 4.3) was not
identified until after the experiments being described here were designed
(and one had been run). This has led to much less data on this topic
being collected in the experiments than would now seem desirable. The
only quantitative data which were suitable for analysis in this area were
the number of separate occasions of inter-unit coordination noted in the
attack experiment responses. There was uniformly no more than one coordi-
nation point at the company level (a line of departure), and in the
responses which contained information at the platoon level, there was no

more than one coordination point at that level, with more than 80% of
the responses showing no coordination of platoons. In view of the fact
that the data showing the importance of coordination down to the indi-
vidual weapon system level had just become clear prior to the Armor
School experiment, some qualitative verbal exploration of the types and
amounts of coordination which might be expected was undertaken at con-
venient points during the experiment. The results of this are discussed
in section 4.6 on miscellaneous observations.

4.6 *Miscellaneous observations*

In view of the exploratory nature of the experiments being run, it
was planned to make notes of any items that struck the investigators as
interesting or unexpected both during the experiments proper and during
the data analysis phase. Many of the observations made seemed to have
no significant impact on the research problem when they were reviewed
later, but a few seemed worthy of reporting, and are therefore described
in this section. Because of the subjective and informal nature of the
observations involved, their possible significance cannot be assessed.

In discussions with the experimental participants at both schools,
investigators attempted to explore the question of how deployment and
maneuver control decisions were actually made in the field. This topic
was explored in some depth at the Armor School, since the mathematical
analyses reported in part I of this report had just reached the point
where the apparent importance of detailed control of the coordination of
exposure was becoming clear.

At both schools, participants described most of these decisions as being made by platoon and only rarely company commanders. In the defense experiment, final decisions on individual weapon system siting were described as being made at the lowest possible command level, so that the commander could actually visit the terrain sites involved. Specific comments were made that adequate site decisions could not be made even from high resolution maps. The process of siting was described as sequential, with a first phase in which general areas for individual weapons or teams were selected on a partially coordinated basis, consideration being given at this stage to the use of the force to provide total coverage of an area in front of it. A second stage would follow in which individual weapon locations would be optimized with respect to the role or sector of responsibility that the weapon had been assigned in the first phase.

At the Armor School, the impressions that the experimenter received from his conversations were that decisions governing movement and other tactical behavior are taken:

(1) by the highest level commander present when a formation unit is in essentially march formation, and

(2) by individual vehicle commanders and occasionally by platoon commanders when a unit is deployed and advancing on or engaging the enemy.

The picture that evolved seemed to be one in which both information gathering and decision-making capabilities at all levels are simply overloaded during combat operations, and decisions must be taken immediately by individuals whose behavior is directly affected.

Although this was the final picture with which the experimenter came away, the evidence from his conversations was not at all uniform in supporting it. Several experimental participants indicated that commanders up to even the company level could expect to coordinate the movements of individual vehicles under their command. A few participants even indicated that they would expect this to occur even if no radio communications were available, with coordination achieved by hand signals.

In view of the degree of conflict in the opinions presented in the conversations, there is a significant possibility that the experimenter's summary picture of the results may have involved a fitting of the evidence to preconceptions or to the first data obtained. No quantitative or replicable evidence exists from which a truly scientific examination could be made. As was indicated in part I, the question is actually much more suited to field experimentation than the kind of map exercise experiment which was conducted in this study.

Turning from the question of coordination and control to a methodological issue, it was found that the line-of-sight overlays which were provided the officers (which, as described in chapter 3, contained line-of-sight data neglecting the effects of vegetation) were uniformly unused in the first (defense) experiment and were accordingly not even taken to the second experiment. Most of the officers made an attempt to use the overlays on the terrain case for which they were provided, but found that without vegetation information begin included in the overlay, they could do what they believed was at least as good a job of analysis with the map alone.

The fact that a fraction of the attack scenarios involved flank attacks was mentioned in the discussion of attacker route selection in section 4.4. These cases typically involved a hook shaped attack route or routes, which proceeded generally perpendicularly against the overall defender line, but aimed between defender concentrations, making a turn to aim at one of the concentrations as it reached a point directly between the concentrations, at which point it would be 500 to 1000 meters from the defender units. Because it seemed to the experimenters that such attacks might have somewhat different properties than those of the more frontal attacks commonly used in the Army study scenarios of their experience, it was felt worthy of remark.

The final observation of this section explains the omission of any discussion of the differences in planned and unplanned attacks in this chapter. As is apparent from the experimental materials in appendix B to chapter 3, attack experiment participants were asked to provide information on both planned attacks against suspected defender locations and unplanned encounters with enemy forces which were not suspected to be present in the area in which they were actually found. The responses to the unplanned encounter information were brief, and generally fell into two groups: those which stated that the result would be the same as that for the planned encounter and those that stated that a unit which encountered unexpected enemy forces would maneuver around them. It was clear as the data were being collected that the experimental participants felt under some time pressure, and were giving little time or thought to

the unplanned encounter responses. Overall, it is not felt that the data obtained should be regarded as permitting solid conclusions to be drawn from it. In view of this, no analysis of this data was undertaken.

PART II, CHAPTER 5:   CONCLUSIONS

The experiments conducted in this study showed no statistically significant association between individual officers and particular tactical decision behavior on any of the dimensions which were examined. This tends to indicate that there is no need for multiple teams of scenario designers and tactical decision makers in Army studies, but that the decisions of a single team on multiple terrains can be expected to reasonably cover real-world variations in tactical behavior. As was pointed out in Part I, this does not mean that a single terrain or scenario is adequate.

The study did show a wide variability in the individual and total responses to the problem of organization of the experimental task forces for combat, even though there was no significant systematic inter-personal variation. This result suggests that Army study methodology might be improved by taking the variability of combat organization into account rather than the currently common technique of assuming that the performance of average or typical organizations adequately describes total performance of a force.

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20. ABSTRACT *(Continue on reverse side if necessary and identify by block number)*

This report describes research concerned with two topics in the broad area of the interactions of terrain and tactics in small unit armored and mechanized combat engagements. One part, described in part I of this volume, addressed the problem of identifying specific quantitative elements of tactical exposure which predict combat results (as estimated by combat models). The second part, described in part II of this volume, examined the degree to which systematic interpersonal variations in tactical decision making may cause —

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Item 20, Continued

variations in engagement conditions, and consequently in combat results.
The results of the first part strongly suggest that fine details of the control and coordination of the exposure periods of the individual weapon systems engaged in a battle are a principal determinant of the predicted combat results of the engagement. The results indicate the need for further research in the area of detailed tactical control of fire and maneuver, including field experimentation concerned with the feasibility of and benefits to be derived from increased degrees of maneuver coordination in small unit armored or mechanized combat. The results of the second part of the research, which involved experiments conducted with Army officers at the Armor and Infantry Schools, show a large variability in the design of tactical organizations for combat, but no significant systematic officer-to-officer variation in this or any other significant tactical variable examined. The results suggest some minor changes in Army study methodology to improve validity.